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ORIGINAL ARTICLES.

I.—Creechbarrow in Purbeck.

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I. Introductory.

THE Isle of Purbeck possesses numerous points of geological interest, and its coast scenery has been celebrated, in conjunction with that of Lulworth, by many writers and artists. The classical work of Sir Henry Englefield, assisted by Webster, in the early part of the last century, served to make known some of the most interesting features of the coast, such as are more or less obvious to all who venture to sail beneath its cliffs. Of late years the geology of the Isle of Purbeck has attracted the attention of the officers of the Geological Survey, and has come in for a considerable amount of description at the hands of H. B. Woodward, Aubrey Strahan, and Clement Reid, in their respective departments. The zones of the Chalk in this region have also been admirably described by Dr. Rowe and his coadjutors, mostly from coast sections.

The interior of the Isle of Purbeck has likewise attracted a fair amount of attention, and those who have participated in some of the excursions of the Geologists' Association during the last quarter of a century will not be altogether ignorant of its leading features.

One of the chief of these features is the long 'Purbeck Hill,' consisting of a narrow outcrop of Chalk, tilted at a high angle towards the north, which stretches from Arish Mell Gap, just underneath Lulworth Castle, on the west, to Swanage Bay on the east. The celebrated 'overthrust' fault, the subject of many a drawing and photograph, is seen in the middle of the Chalk formation between Ballard Point and Old Harry on the northern horn of Swanage Bay, which also forms the most easterly point of the Isle of Purbeck. The effects of this fault, or axis of maximum tension, may be traced for the most part along the northern slope of the Purbeck Hill, though its effects are not so obvious as in the Lulworth district further to the west. The principal fact to be noticed in this connection is that the so-called Purbeck Hill may be said to divide the Isle of Purbeck into a southern and a northern

area, and it is to this latter region, which is wholly composed of Tertiary strata, that I would direct attention.

The maximum elevation of the Purbeck Hill is about 650 feet, and its average height might be somewhat under 500 feet; it is breached at Corfe Castle, whence the drainage of part of the southern area escapes through a sort of double gap in the Chalk to flow across the low-lying Tertiary area to the northwards. Practically, with this exception, the Purbeck Hill presents an unbroken range of considerable steepness to the Tertiary district comprised within the Trough of Wareham, and which, as far as the river Frome, is regarded as forming a portion of the Isle of Purbeck. There is, however, a remarkable object which breaks the somewhat monotonous outline of the long Purbeck Hill, and that is the conical figure of Creechbarrow, which rises, all alone, at a little distance northward to a height of 637 feet, and is composed, not of Chalk, but of Tertiary strata. Seen from certain points Creechbarrow looks like a pimple on the Chalk ridge itself, whilst from other positions its peculiar outline greatly resembles a volcano, and when there happens to be a gorse fire near the summit this impression becomes almost a reality.

Geologists will readily understand that there is nothing igneous about Creechbarrow, but for all that its origin is extremely enigmatical. Why a mass of Tertiary beds should stand out, like a sentinel, in front of a long line of Chalk hills, rivalling these Chalk hills in height, whilst the bulk of the Tertiary strata in the neighbourhood are almost wholly below the 300 feet contour, was always a puzzle to me, as it evidently was to the earlier members of the Geological Survey. Day after day and month after month at intervals during the past four years have I noticed that graceful hill under every conceivable atmospheric condition, until the desire to account for its exceptional presence grew stronger within me.

Animated by these sentiments I succeeded in inducing the chiefs of the Dorset Natural History and Antiquarian Field Club to arrange for an excursion to Creechbarrow last Summer. The Fates accorded us a beautiful day, and the "West Purbeck Meeting" on the 21st of August last was attended by something like 100 people. Lord Eustace Cecil presided in the absence of Mr. Mansel-Pleydell through illness, and under his auspices I endeavoured to expound my views from that airy spot. My remarks are fully recorded in the Proceedings of the Dorset Field Club.[1]

Amongst other points that were urged I endeavoured to impress upon the meeting the exceptional character of Creechbarrow, and for this purpose I entered into the physical description and history of the Purbeck Hill, the Trough of Wareham, etc., at some length.

Since we had the map of Nature before us there was no need of a diagram, but in order to bring the subject home to those at a distance I submit the accompanying diagrammatic sketch of the Trough of Wareham.

[1] Vol. xxii, p. liv.

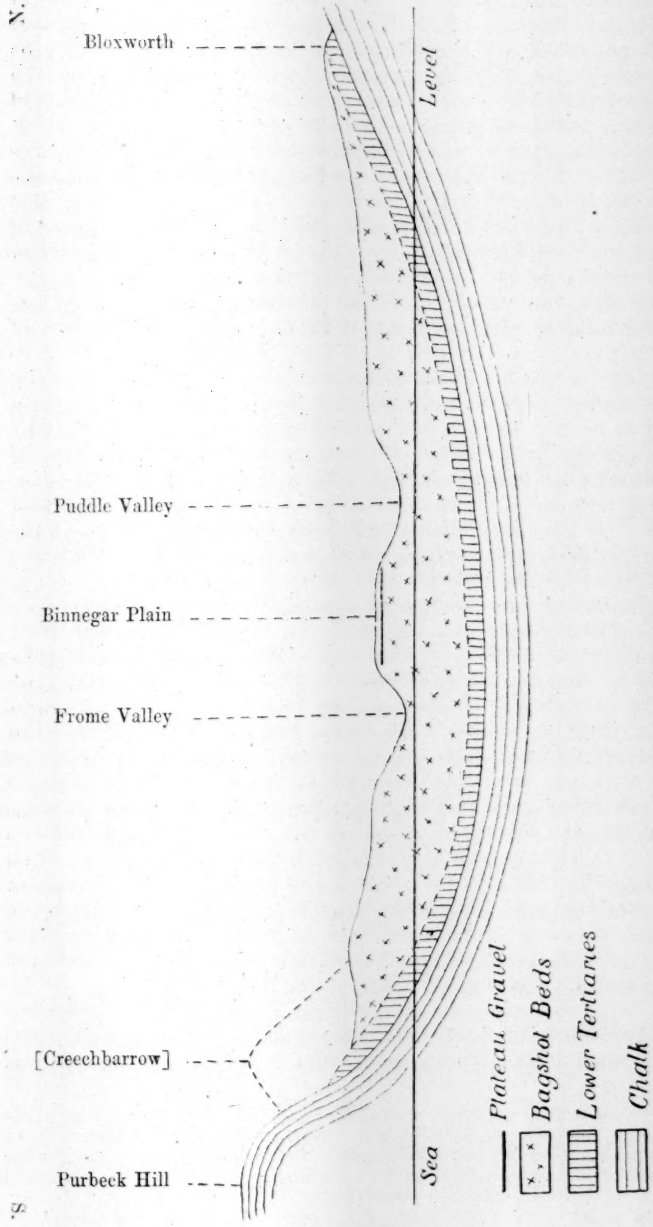


Fig. 1.—The Trough of Wareham.

Note.—If this diagram were continued in a northerly direction it would be found that the Chalk ultimately rises in the Dorset Downs to heights considerably exceeding that of the Purbeck Hill, thus completing the figure of the great Dorset syncline.

This diagram, which has an horizontal extension of about 8 miles from south to north, is intended to represent the *general* topographical and geological structure of the Trough of Wareham on a meridian both east and west of Creechbarrow. The section itself may be taken more particularly to represent a meridional line to the westward of that hill drawn through West Creech, Binnegar, and Bere Heath. Accurate detail is not attempted, but the thoroughly exceptional character of Creechbarrow can be gathered from the dotted outline of the hill itself, projected on the section. We perceive at a glance that the Tertiary beds in this hill are just twice as high above sea-level as they are in the *normal* or *general* section between the Purbeck Hill and the Frome River. The latter is the northern boundary of the Isle of Purbeck.[1]

Having thus impressed upon the meeting the thoroughly exceptional character of Creechbarrow in its relation to the rest of the Tertiary beds in the neighbourhood, I next endeavoured to adduce some reasons for the explanation of this anomaly. Premising that hills, as we see them now, are the outcome of certain elevating forces of a remote period, and that they have been fashioned by meteoric agencies, which themselves are modified by various internal and external peculiarities, I offered three possible suggestions or theories to account for the existence of Creechbarrow as we now see it, and for its exceptional relation to the Bagshot Beds of the immediate district, which largely consist of potter's clays of different qualities (the Pipeclay beds).

(1) The first of these suggested explanations may be summed up under the heading "Exceptional development of the Creechbarrow Beds themselves." It is well known what valuable workings in the clays at the foot of the Purbeck Hill have been carried on for nearly a century. This has been one of the most important industries of East Dorset. We may trace the line of the old workings exactly as if drawn on a map all the way to Corfe Castle. An old workman who had long been engaged on this business pointed out to me some time ago that *Creechbarrow bulges all these beds and throws them out of line.* Such a state of things tends to prove that there is something peculiar in the original composition of this monticle. It may be inferred also from the above statement that the Creechbarrow Beds contain no potter's clay. An inspection of the map showing the curve of these workings will prove at once how correct was the old workman's remark about the bulging of the Pipeclay series.[2] So much for theory No. 1.

(2) The second explanation, or rather suggestion, had reference to the proximity of the great thrust-plane which governs the tectonic constitution of so much of the Isle of Purbeck. "It is

[1] The general structure of the Trough of Wareham is no doubt that of an undulating syncline, but owing to the softness of the Bagshot Beds accurate stratigraphy is not attainable. I am inclined to believe that the original bottom of the syncline is represented by the Plateau-gravel on Binnegar Plain, and that both the Frome and Puddle Valleys have been excavated subsequently.

[2] Map to accompany the forthcoming paper in the Proc. Dorset Field Club.

represented in the geological map as passing along the junction of the Chalk and Tertiaries about 300 yards to the south of our present position [i.e. on Creechbarrow]. It is quite possible that, instead of sticking to that route, the line was deflected somewhat, so that the thrust-plane has come in among the Tertiary clays. . . . The great thrust from the north has pressed these clays against the hard Chalk, making them harder and fitter to withstand the forces of denudation."[1] Remarkable instances of derangement of the strata and reversal of dip have been experienced in the pipeclay workings, and the Chalk itself is found to be dipping to the north at an angle of 80° at the limekiln which is close to Creechbarrow. In consequence of such evidence of the effects of the Isle of Purbeck thrust-fault in this immediate district, it occurred to me at that time that exceptional tectonic forces, locally exerted, might possibly have had something to do with the formation of this peculiar hill. So much for theory No. 2.

(3) The third explanation offered was the most theoretical of all, as it involved a spread of Plateau-gravel on the summit of Creechbarrow at some period of its history. Everyone knows how largely the Plateau-gravel has contributed to the formation of high ground, more especially in the Bagshot districts of Surrey, Hants, and Dorset, so that what was originally a shallow valley, owing to the protection afforded by an accumulation of flint gravel, often cemented by iron-oxide, becomes relatively high ground and sometimes even a hill.[2] On this occasion I went so far as to express my belief that there had been Plateau-gravel on the top of Creechbarrow. The principal evidence for this hypothetical bed of gravel arises from the fact that about a thousand yards from the summit of the hill on the north side, near the 300 feet contour, there is a large deposit of clayey gravel, which is worked for road-metal and probably for other purposes. This bed of gravel, it then appeared to me, might be the remains of a deposit once at a much higher elevation, which had, to a certain extent, been slid off these slippery clays, especially during a period when there was a great deal of snow, which would facilitate deposit on the north side of a slope. Another indication of this supposed gravel bed on Creechbarrow, which was then present to my mind, arises from the great number of peculiarly large flints which may be seen about the hill. They are, for the most part, unabraded flints, and their position is such as to suggest that many of them may have rolled down from the upper part of the hill, though they are now to be noticed principally on the eastern base. These loose flints I held as additional evidence of the existence of my hypothetical gravel bed on the top of this hill. So much for theory No. 3.

[1] Op. et vol. cit., p. lviii.

[2] The valley-flat of days gone by
Becomes the plateau, high and dry:
Thus, in the cycle of the Ages
The Drifts reverse their earlier stages.
ANON.

The address concludes with the following remarks:—

"But we cannot say what the hill itself consists of, for the whole hill, especially the top of it, has been thoroughly sophisticated. I have not the remotest idea of what the original surface of the summit consisted. This tumulus, which forms such a convenient shelter, is composed of those very large flints which have been gathered from all around. But the greatest annoyance is that a house has been built on the top of Creechbarrow, and the foundations and walls make it utterly impossible for anyone to judge what may have been the original composition of the summit. It has been sophisticated by man, and the only way I can see of ascertaining the true structure of Creechbarrow is to drive a horizontal level right in to see what the hill is really made of. If the Dorset Field Club would like to vote a sum of money for that purpose, I will undertake to see that it is properly spent. But I am afraid that this is hoping rather too much."[1]

II. Investigation.

Acting on my own suggestion, as conveyed in the preceding paragraph, having first obtained the permission of the owner and occupier, I set some men to work digging in October last, and we very soon arrived at most unexpected results. The remarkable greensward on the very summit of Creechbarrow, so different to the vegetation of the typical Bagshot districts, where heather and Iceland moss afford a meagre diet to the rabbits, had always provoked my suspicions. No one, however, had ever suspected that the actual summit of Creechbarrow consists of LIMESTONE, but now we perceive that the calcareous nature of the soil is chiefly accountable for this unwonted greenery at such an elevation.

Altogether some seven or eight pits were dug within the limits of the 600 feet contour. It is not intended to specify in the GEOLOGICAL MAGAZINE the particulars of these, but it is hoped that such details as may be of interest will appear in the forthcoming volume (No. 23) of the Proceedings of the Dorset Field Club. The first pit (No. 8*a*)[2] was dug in the eastern flank of the summit ridge, some distance below the actual summit. Here a calcareous talus was encountered before the solid geology of the hill was reached. This calcareous talus created great surprise, and many were the conjectures as to its origin. The material was mainly in a chalky condition, and as there were no fossils to guide us we concluded that some chalk had been brought up to the top of the hill. Then, again, remembering that the foundations of the house formerly existing on the summit consisted of dressed Purbeck stone, we fancied that a sufficient quantity of Purbeck stone might have slipped down to form a talus—and this seemed the more probable, as the less perished stones had rather the look of a fresh-water limestone: so the theory of a Purbeck, or even a Portland origin, prevailed over the original supposition of derivation from the Chalk. At that stage of the investigation we never dreamt

[1] Vol. cit., p. lix.

[2] These figures refer to the numbering ultimately adopted.

of an autochthonous deposit of limestone on the hill itself. When the calcareous talus was penetrated in this section, we found the solid geology to consist of a peculiar cakey sand, passing down into buff-coloured sands with vertical tubes having a calcareous lining. Underneath this was an irregular layer of flints, some of great size, about a foot thick, and lower still loose white sand, preceded by yellow sandy clay blotched with oxide of manganese and containing perished flints.

So far as I know, these yellow manganese-stained sands and clays, in conjunction with the flint beds and the concretionary limestone subsequently discovered, make up the hill-top of Creechbarrow, that is to say, of the area within the 600 feet contour, some three or four acres in extent. Since the source of the limestone fragments in the talus had not yet been discovered, we made an excavation a few feet higher up (No. 8), and there we had the satisfaction of detecting a massive tufaceous limestone with an irregular surface passing downwards into sand with calcareous concretions, and this excavation was carried down to a layer of flints which was not pierced. Whether this is the same as the layer in the lower pit one cannot say.

Before proceeding to describe the complete discovery of the limestone, I will draw attention to the problem suggested by the flints. It will be remembered that when in August last I offered three possible suggestions as to the origin of Creechbarrow, I laid considerable stress on the abundance of flints knocking about over the hill, as evidence of the former existence of Plateau-gravel upon or near the summit. It was now proved that these large and peculiar flints which one associates with Creechbarrow are not by any means derived from any superficial deposit, but that they constitute an important part of the beds of which the hill is composed. These beds are undoubtedly of Lower Tertiary age, and if we assume, in default of evidence to the contrary, that they are of Bagshot age, at least these flint beds constitute a portion of the series, whatever it may be. But in order not to prejudge the question of age, I will simply speak of the whole series as the *Creechbarrow Beds*.

In describing certain sections further on the subject of these bedded flints will again crop up, but we may note some peculiarities with reference to them now. It must always be borne in mind that these flints are large stones in the midst of extremely fine sediments. There are, no doubt, throughout the Creechbarrow Beds, as elsewhere in the Bagshots, coarse iron-grits and lydite gravels, such as the people in the neighbourhood call 'granite gravel.' Still, the bulk of the deposits, whether of sand or clay, are of fine material, and yet these large flints occur in abundance, not only disseminated occasionally through the clays and sands, but accumulated in beds of varying development up to 4 feet in thickness. These flints have for the most part a creamy appearance, they are much degelatinized, and in some cases the exterior is simply a mass of granular silica, very meagre to the touch. They are also extremely brittle when first dug out, though I rather think that exposure to the atmosphere toughens them after a while. In consequence of

this brittleness the available fragments do not much exceed 28 lbs. in weight, so far as I have seen them, though it may well be that much heavier flints than these occur. The surface of those flints which are not much broken has been subject to very little modification from abrasion. Associated with the big flints are flint pebbles and other stones of moderate size, also quartzose grit.

Having ascertained the undoubted existence of indigenous limestone on Creechbarrow, it became necessary to obtain some further knowledge both of its mode of occurrence and general character. Pits were opened in one or two places, and especially on the summit. Before proceeding to describe the summit pit (No. 5) I will submit the section of No. 4 pit, which is on the south slope of the actual summit and forms part of the escarpment (see Fig. 4, p. 255).

The following sequence was observed:—

	ft.	in.
1. Soil with few flints, fragments of pottery, oyster shells, etc.	1	6
N.B.—A deposit of this nature forms a sort of talus to the whole section on the hill-side.[1]		
2. Softish tufaceous limestone	2	6
3. Passing downwards into sands full of calcareous concretions	4	0
4. Band of yellow clayey sand	0	6
5. Loose buff-coloured sands	5	10
6. Layer of large flints of the Creechbarrow type ...	1	0
7. Pale buff-coloured sands (not bottomed)	4	0
Total	19	4

For convenience this section was cut in three steps. It was here that the principal evidence for dip was obtained. No bedding was observable in the tufaceous limestone, and the irregular nature of its base causes it to be of little use for stratigraphical purposes. The thin band of "yellow clayey sand" was more useful, and also the "flint layers," so that from these and other indications an inclination of 9 inches in 48 inches was observed, the direction being somewhat to the west of north, or at any rate northerly. This may be calculated into an incline of 1 in $5{\cdot}5 = 11°$ nearly. The identification of the particular flint band in another pit, supposing such identification to be correct, serves to confirm the amount of dip and also the direction. The longer axis of the hill itself runs in a direction about N.N.E., being nearly at right angles to the trend of the long Purbeck Hill, and this peculiar conformation is especially noticeable from many points in the Frome Valley. The great bulge which the Creechbarrow Beds impress upon the Pipeclay series has the same direction as the longer axis of the hill. Hence the observed dip and the dip slope (see Fig. 3) are slightly divergent. On the whole, without attempting an accuracy which in beds of this

[1] As regards these deposits in the soil and in the semi-artificial talus, I have the following note:—Black soil with pottery, oysters, limpets, mussels, periwinkles, bones, fragments of Purbeck stone (dressed), iron sand, pieces of the hill limestone, old pipes, etc.

character is practically impossible, we may say that the Creechbarrow Beds, as determined from indications near the summit, have a dip to the northwards of from 10° to 12°.

Fig. 2.—The Summit Pit (No. 5). From a photograph by Mrs. Hudleston.
A. Solid concretionary limestone *in situ*.
B. Artificial rubble, mainly derived from the limestone.

The vertical section as shown in Fig. 2 is about 7 feet.

The summit has been artificially flattened, and certain courses of Purbeck stone (dressed) remain as evidences of the foundations of the Keeper's Lodge which formerly occupied this site. About a foot or eighteen inches below this we came upon a section which is partly natural and partly artificial. The natural portion consists of solid tufaceous limestone, showing certain irregular divisions, but nothing that could be referred to bedding: the jointing is in all directions with possibly a tendency to the vertical. Alongside of this mass of natural limestone is a miscellaneous rubble of artificial origin. The upper portion consists of small calcareous rubble mixed with sand, flints, etc., down to a line of blackearth. Below this is a conglomeration of looser stones, where flints and large blocks of tufaceous limestone may be noted. The whole probably rests on tufaceous limestone *in situ* lower down.

Situated on the very top of Creechbarrow this section is very suggestive. That the summit has been artificially flattened for the purpose of supporting a structure is evident. At what period this was *first of all* done it is impossible to say, but we are justified in supposing that before human interference with such a prominent feature of the landscape, the limestone rock stood out like a needle or tooth above the encompassing greensward. Being too precipitous in its natural state to support a building, masses of the original summit have been cut away and used in forming the strange medley of rubble which is seen on the right of the section. We perceive indeed the very fragments of limestone that have been detached so as to make a platform on which to build. The foundations are thus partly natural and partly artificial.

It is possible from specimens obtained in this pit more especially to form some estimate of the character of this limestone. I hope to treat the subject more fully with appropriate illustrations in the Proceedings of the Dorset Field Club. Meantime, the following rough lithological notes extracted from my field notebook will serve to give an idea of some of the specimens.

2*a*. This is a large fragment of creamy white tufaceous limestone with specs and threads of oxide of manganese in places; flattened pisolitic bodies in brownish calcite are numerous. There are casts of interiors of a shell not unlike *Paludina*. The whole of this fragment has a tufaceous aspect and is free from the buff-coloured patches which characterize a certain group of specimens.

The external surface is rough and in one corner full of curious shapes, which are probably concretionary bodies developed by weathering.

2*b*. A white tufaceous limestone similar to the last. This is pretty full of pisolitic bodies in brown calcite; it moreover contains a peculiar *horse-shoe section* in brown calcite, of which there are indications in so many of the specimens. There are indications of a *Paludina* likewise, but so merged in the matrix as to be indistinct.

2*d*. Similar to the last and containing the indications of a *Paludina* which clearly differs from the ordinary Purbeck species (*P. carinifera* and *P. elongata*).

3*a*. A creamy tufaceous limestone with some buff-coloured patches and specs and threads of manganese oxide. Sections of ordinary pisolites here and there. But this specimen is remarkable for three very large *horse-shoe sections*, which certainly represent concretions in brown calcite, whose origin is by no means clear. These sections show a series of concentric rings with a large hollow in the centre filled with the ordinary matrix. There is no radial structure; one end of the circle is thick, whilst the opposite end thins almost to disappearance. Some of these horse-shoe sections are nearly $1\frac{1}{2}$ inches in diameter.

3*c*. Rather more compact limestone with some buff markings. The chief interest of this specimen is, that it seems to afford a solution of the problem of the horse-shoe sections. There is to be seen here a pisolitic concretion with an interior *like a very small egg*, of which

the shell, represented by the brown calcite, is developed so obliquely, that it is quite thick on one side and quite thin on the other side. A section of this gives a shape like a horse-shoe, which evidently belongs to the group of pisolites.

4*a*. Very fine specimen of pisolitic limestone, quite suitable for collections. Cuts well and takes a good polish.

Without special illustration the above may be accepted as a brief lithological description of the Creechbarrow limestone. Recently some better specimens of univalves have been found, and these are free from matrix; but on the other hand they have suffered from an incrusting process which has greatly affected their original form. The *Paludina* appears to be a short variety of *Paludina media*, and the other univalve may possibly be a member of the Melaniadæ.

This limestone forms the summit of Creechbarrow, and it is terminated towards the south by what may be deemed an escarpment (see Fig. 4). We can follow it on the dip towards the north, though its lateral extension east and west of the immediate summit is certainly restricted, but the elongated shape of the summit ridge is undoubtedly determined by this very hard rock. About 120 feet N.N.E. of the summit, in the direction of the dip slope, we sank another pit (No. 6) and came upon a very interesting section. The hard nodular limestone is here found to form a very rough and irregular surface, beneath a peculiar deposit which has a depth of from 3 to 5 feet according to the inequalities of the limestone. About 100 feet from this in a N.N.W. direction, and just within the 600 feet contour, we found this same peculiar deposit, quite 6 feet thick, resting on the irregular surface of the tufaceous limestone, here apparently undergoing a softening process.

The deposit on the Creechbarrow Limestone.—Of the many enigmas which this curious hill presents to the puzzled geologist there are none more difficult to solve than the true nature and origin of the deposit which, from a geological point of view, is the highest in the sequence, although so irregularly distributed that it has not been indicated on the dip slope section (Fig. 3). The upper portion of the deposit, in places, consists of a mass of rounded and subangular flints in a muddy matrix, passing downwards into a stiff yellow clay with much black oxide of manganese, and a variable assortment of large and small flints, flint pebbles, and other stones, disposed after the manner of Drift. At present I must be content to state the facts about this deposit as I find them, without attempting any explanation. It seems probable that in spite of its superficial position this remarkable drift is really part of the Creechbarrow series, and does not in any way represent the Plateau-gravel.

We must now endeavour to trace the limestone (*a*) on the dip slope to the point where it disappears. Omitting any indication of the peculiar deposit mentioned above, Fig. 3 is a rough superficial section from the summit of Creechbarrow (637 feet) to the sand-pit about the 350 feet contour.

The horizontal distance may be 1,150 feet. The northern spur which we follow is in alignment with the longer axis of the hill,

which extends N.N.E. from the summit. It must not be supposed that the calcareous beds have been proved every yard of the way. There are two places on this spur, above the 500 feet contour, where 'marl' has been found close to the surface. The highest of these is 250 feet, measured horizontally from the last pit where we found the

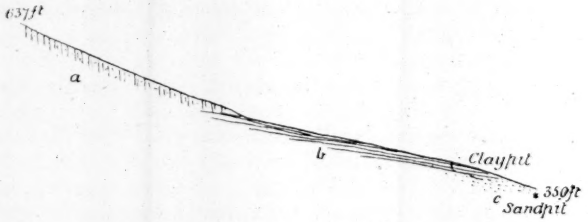


FIG. 3.—The Northern Slope of Creechbarrow.
a. Hill-top, or Creechbarrow limestone. *b*. Brown Clay series.
c. Sands underlying the Brown Clay.

hard hill-top limestone. I should observe that the term 'marl' merely refers to a softened condition of the limestone. There is a well-marked bluff about the 500 feet contour, near where the calcareous series terminates, and the following section, derived from a series of trenches which we made in the hill-side, shows the nature of the Creechbarrow series at this point. The top of the section just reaches the 500 feet contour.

NORTHERN SECTION.

	ft.	in.
a. Surface with tree roots, etc.	3	0
b. Fine clayey sands with flints	2	0
c. Sandy without flints	2	6
d. Tough yellow clay	2	0
e. Upper marl	1	0
f. Tough clay	2	6
g. Main marl	7	0
N.B.—There is a break here owing to old workings, which would add to the marl ...	5	0 ?
h. Impure marl containing sand—a passage bed ...	3	3
i. Buff clayey sand	1	6
j. Flint gravel and sand	1	3
Total section	31	0

This series of beds rests upon a buff-coloured clay.

(*b*) Is a very extensive and important deposit, the thickness of which it is not easy to estimate. It can hardly be less than 40 feet thick and may be more, and constitutes an important feature along the north side of the hill on both sides of the 400 feet contour. In the brickearth are said to be lenticular masses of sand with flints at the bottom, which to a certain extent detracts from its value for brick and tile making. The brown clay series, in which Mr. Bond's clay-pit and brickyard are situated, rests on sands (*c*) which may be well studied at the 'Sand-pit.'

Returning for a moment to the limestone (*a*), it is evident that the face, or bluff, so well marked in the 'Northern Section' is not a natural one: everything has the appearance of an extensive open working, presumably for 'marl,' yet there is no tradition as to any working here, consequently the period must have been sufficiently remote. The continuation of this face westwards, usually near the 500 feet contour, also has an artificial look about it indicative of former workings. One of the most peculiar circumstances in connection with this case is the total absence of any limestone fragments throughout this long line of presumed old workings. The same remark must also be made as to Creechbarrow generally. Had any limestone fragments been found rolling about, the discovery of the limestone must inevitably have followed. I have searched also in old walls and houses for any trace of this limestone, without success. Where stone has been used in these buildings it seems in all cases to be Purbeck stone. Consequently we are faced with the fact that while flints of the Creechbarrow series abound and are characteristic, no single specimen of the Creechbarrow limestone appears on the surface. Rapid disintegration when exposed to the atmosphere can be the only explanation, though in view of the excessive toughness of the hill-top limestone this seems all the more extraordinary. There certainly are some peculiarities in these concretionary limestones which we do not fully understand. I mentioned before that in the talus near the summit the limestone was already softened to the consistence of chalk, and now we perceive that in the course of its further descent it passes away altogether as a distinct rock.

III. Inferences.

There are many points yet to be considered in connection with Creechbarrow, but the facts detailed in the preceding pages must suffice at present for a communication which is essentially preliminary. The great problem of all, viz., the stratigraphical relation between the Creechbarrow Beds and the Pipeclay series of recognized Bagshot age, is as yet uncertain. This is a matter of local interest, requiring special illustration in the way of plans, etc., which I hope to bring forward in the Proceedings of the Dorset Field Club. It is certain that the Creechbarrow Beds press the Pipeclay series towards the N.N.E., or, in other words, they are said to 'bulge' them. This circumstance is in favour of their being of Bagshot age, and on a lower horizon than the Pipeclay series. On the other hand, the occurrence of limestone in the Bagshots is, so far as I am aware, a phenomenon hitherto unknown in England. Moreover, there is a certain degree of analogy between the Bembridge limestone and the Creechbarrow limestone, which at least renders it possible that the latter may really belong to an Oligocene series. Hitherto it has been a matter of exceeding difficulty to ascertain what are the stratigraphical relations of the Creechbarrow Beds to the Pipeclay series. If the Creechbarrow Beds are of Oligocene age, then there is no great difficulty in supposing that they simply overlie the whole Bagshot series and are cut off on the north. But if they are of Bagshot age,

as I consider most probable, they must pass under the Pipeclay series, or in some unexplained manner abut against them.

It should be borne in mind that the whole development of the Creechbarrow Beds is totally different to the Pipeclay series of the Bagshots. The following points are among the differences as shown in the Creechbarrow Beds: we have a concretionary pisolitic limestone of fresh-water origin, an abundance of large flints and other coarse detritus in intimate association with fine yellow clay and sands (I am informed that no flints are found in the Pipeclay beds). There is also a considerable amount of oxide of manganese, especially in the mechanical sediments, and the occurrence of botryoidal nodules of this mineral is noted in some of the sands. Plant-remains and carbonaceous matter are also rare. On the other hand, the Pipeclay series is remarkable for the complete absence of calcareous matter, the excessive fineness of its sediments, the large development of pale clays with admixture of stained or 'bloodshot' clays, and the abundance of carbonaceous matter, sometimes with plant impressions and remains.

One point of considerable importance, which can only be brought out fully with the aid of a sketch-map, consists in the fact that, whereas immediately to the east of Creech Farm the Pipeclay series is found almost in contact with the London Clay and so continues all the way to the gap at Corfe Castle, yet *to the west* of this line, in the vicinity of Creechbarrow itself, we find that a distance of several hundred yards separates the Lower Tertiaries from the Pipeclay, the intervening space being occupied by the Creechbarrow Beds. At present I can only state the fact without offering an adequate explanation. We can scarcely believe that the whole Creechbarrow series has been faulted out in the area where it is missing, and thus we must fall back on the theory of exceptional development within a limited area if we adopt the Bagshot age of this very peculiar set of beds. We know from Mr. Clement Reid's memoir that the Bagshot Beds west of a certain meridian in the county develop into exceedingly coarse sediments, but there is nothing of this kind in Purbeck, excepting the Creechbarrow Beds themselves, which are only of limited extent.

Let us now see how this investigation bears on my three theories as to the origin of the hill itself. Practically these were: (1) original peculiarity of deposit, (2) exceptional tectonic disturbance, (3) protection afforded by Plateau-gravel.

Of these three the first is nearest the mark, and the third may be entirely dismissed. The whole area has, of course, been greatly affected by tectonic disturbance, but it is doubtful at present whether anything exceptional of this nature has contributed to the formation of this peculiar feature of the landscape.

The accompanying section will serve to show the relation of the mass formed by the Creechbarrow Beds to the Lower Tertiaries (London Clay and Reading Beds) and the Chalk. One important point to bear in mind is that the neck of land, consisting of Lower Tertiaries, which connects Creechbarrow with the Chalk, has an

elevation of 500 feet, whereas the Lower Tertiaries along the dip slope of the Purbeck Hill elsewhere rarely attain an elevation of 300 feet (see Fig. 1 for confirmation of this). Hence, the agency which protected the Creechbarrow Beds protected the Lower Tertiaries to a certain extent. I cannot doubt that this agency was the hard concretionary limestone which now constitutes the summit of Creechbarrow itself, and which in all probability extended considerably further in a direction opposite to the dip after the manner of escarpments generally.

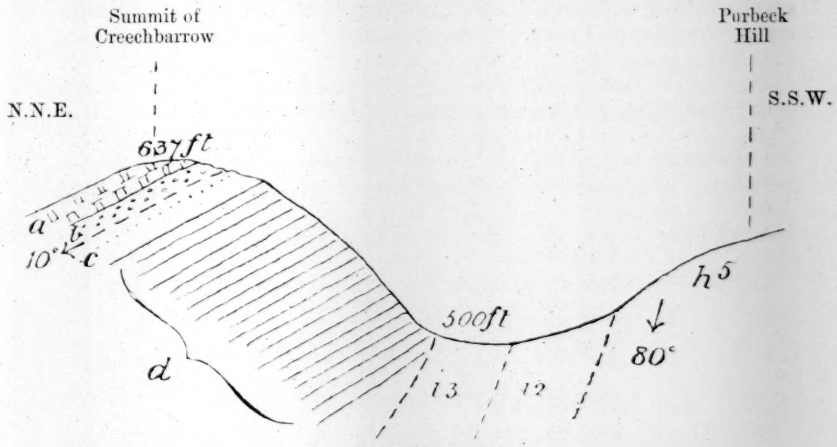


FIG. 4.—Generalized section of Creechbarrow (south side).
a–d. Creechbarrow Beds. *a*. The hill-top limestone. *b*. Sands with calcareous concretions. ⇐ Bed of flints. *c*. Sands below the flint-bed. *d*. Beds not specially determined, mostly sandy. (See section-page.)
i^3. London Clay. i^2. Reading Beds. h^5. Chalk.

Whether the Creechbarrow Beds are really unconformable to the Lower Tertiaries, as shown in the diagram, I am unable to say. Also the angle of dip of these beds is only obtained by inference. The high dip of the Lower Tertiaries shown in the diagram is based on the three following considerations: (1) on the narrowness of the outcrop, (2) on a dip of 80° towards the north which is seen in the adjacent chalk-pit, and (3) on the fact that one of the ironstone-grits of the Reading Beds is seen in a vertical position about $\frac{3}{4}$ mile to the eastward of the section. With reference to the Creechbarrow Beds themselves, a dip of from 10° to 12° is obtained from observation for the topmost series,[1] but the actual dip of the lower beds, in immediate contact with the London Clay, as also their true stratigraphical relations, can only be a matter of inference.

The primary object of this investigation has been an endeavour to explain the causes which have led to the formation of such an exceptional feature in the landscape of the Isle of Purbeck as Creechbarrow, certainly the most noteworthy hill composed

[1] See p. 248.

exclusively of Tertiary beds in all England. It was originally represented by the Geological Survey as being held up in a forked fault with a sort of repetition of the London Clay at its northern base. There certainly seems no justification for the introduction of the London Clay into this position, viz. the northern base of the hill. There may, however, be some better reason for the introduction of a fault, for it is just hereabouts that the junction of the Creechbarrow Beds with the Pipeclay series takes place. This is a piece of stratigraphy which I have not yet succeeded in solving to my own satisfaction, though such a question has only an indirect bearing on the origin of Creechbarrow as a hill. I am far from saying that everything in connection with its origin has been explained, but without doubt one great predisposing cause is the protection which the softer strata have received from the concretionary limestone. Such protection is not only accountable, in a great measure, for the existence of the hill itself, but also for its present shape, which has to a considerable extent been determined by the original form of the calcareous body that accumulated in some old Tertiary lakelet, ages before the uplift of the Purbeck Hills.

II.—Woodwardian Museum Notes: Salter's Undescribed Species. VIII.[1]

By F. R. Cowper Reed, M.A., F.G.S.

(PLATE XVI.)

LAMELLIBRANCHIATA (Continued).

Goniophora grandis, Salter. (Pl. XVI, Figs. 1, 2.)

1873. *Goniophora grandis*, Salter: Cat. Camb. Sil. Foss. Woodw. Mus., p. 151 (*a* 827).

1891. *Goniophora grandis*, Woods: Cat. Type Foss. Woodw. Mus., p. 77.

There is only the one poorly preserved and imperfect specimen in the Woodwardian Museum on which Salter founded this species. It comes from the Wenlock Limestone of Dudley and belongs to the Fletcher Collection. Salter says of it (*a* 827): "Strongly costated and marked with lines of growth which decussate the ribs. Twice the size of the common Ludlow species." The material is so poor that the specific description must necessarily be incomplete.

Diagnosis.—Shell narrow, transversely elongate, dorso-ventrally compressed. Valves very deep, angulated by diagonal, very strong carina from beak to posterior angle. Anterior side with large deep subcordate lunule. Below the diagonal carina the surface of the valves is marked by about twelve strong radiating rounded ribs, curved slightly forwards, parallel to the margins of the lunule and crossed by numerous concentric growth-ridges. Above the carina the sole ornamentation is the continuation of this series of concentric ridges, the radiating ribs being entirely absent.

Measurement.—Length (diagonal, along carina) approximately 45–50 mm.

[1] For previous articles see Geol. Mag., 1901, pp. 5, 106, 246, 355, and 576; 1902, pp. 122 and 145.

Remarks.—It is not very profitable to compare this poorly defined species with others. The only form of *Goniophora* known to me with similar radiating ribs on the anterior lower half of the shell only and concentric ridges above and below the carina, is *G. truncata*, Hall,[1] from the Hamilton Group of New York.

Modiolopsis complanata (Sow.).

1851. *Modiolopsis complanata* (Sow.), McCoy: Brit. Pal. Foss., p. 266.
1873. *Modiolopsis planata*, Salter: Cat. Cam. Sil. Foss., p. 182 (*b* 71–3).
1891. *Modiolopsis planata*, Woods: Cat. Type Foss. Woodw. Mus., p. 81.

The specimens which McCoy named and described (op. cit. supra) as *M. complanata* (Sow.) were renamed *M. planata* by Salter, though in the margin of his Catalogue (loc. cit.) he puts *M. complanata* with a query after it. I do not see sufficient reason to doubt the accuracy of McCoy's identification, and therefore Salter's specific name *planata* must be dropped.

Modiolopsis mimus, Salter. (Pl. XVI, Fig. 3.)

1873. *Mytilus mimus*, Salter: Cat. Camb. Sil. Foss. Woodw. Mus., p. 182 (*b* 141).
1891. *Mytilus minimus*, Woods: Cat. Type Foss. Woodw. Mus., p. 82.

The name of this species is printed *mimus* in Salter's Catalogue, both on p. 182 and in the index, p. 200. On the back of the tablet bearing the specimen the name *minimus* was found written in pencil, and Mr. Woods adopted this in his Catalogue. There is only one specimen of the left valve (*b* 141), and it comes from the Upper Ludlow of Lesmahagow.

Diagnosis. — Shell inequilateral, obliquely elongate, obtusely pointed. Body narrow, slightly curved, convex, especially near beak. Beak anterior, terminal, small, pointed, convex. Posterior portion of shell rather depressed and flattened, but not sharply marked off from body of shell. Ventral margin short, subacuminate. Posterior margin almost vertical, gently rounded. Hinge-line straight, nearly the width of shell, at angle of 60° to oblique axis.

Surface marked with concentric growth-lines.

Measurements.	mm.
Length along oblique axis	16
Width along hinge-line	9
Length from hinge-line to ventral margin	12

Remarks.—This species bears some resemblance to the Bala form *Modiolopsis pyrus*, Salter,[2] but the hinge-line is relatively rather longer, the beak more acuminate and terminal, and the posterior portion of the shell larger. Some American species of *Leptodesma* from the Chemung Group[3] seem to bear a considerable resemblance to *M. mimus*.

Orthonota (?) Hughesi, Salter. (Pl. XVI, Figs. 4, 5.)

1873. *Ctenodonta Hughesi*, n.sp., Salter: Cat. Camb. Sil. Foss. Woodw. Mus., p. 82 (*a* 979).
1891. *Ctenodonta Hughesi*, Woods: Cat. Type Foss. Woodw. Mus., p. 74.

[1] Hall: Palæont. N.Y., vol. v (1883), p. 13, pl. xliv, figs. 1–5.
[2] Mem. Geol. Surv., vol. iii, 2nd ed. (1881), p. 552, woodcut xii, fig. 1.
[3] Hall: Pal. N.Y., vol. v, pt. 1, pl. xci, figs. 23–5.

Salter says of this form that it is "squarer and more ovate than the kindred Caradoc forms." There are four specimens in the Woodwardian Museum, and Salter based his species on two of them (*a* 979) consisting of a complete internal cast of both valves and an external cast of the upper portion of the shell. All were found in the Lower Llandovery Beds (Upper Bala of Salter) of Sefin Llettyrhyddod, Llandovery.

Diagnosis.—Shell equivalve, inequilateral, transversely subovate, narrowing and subtruncate posteriorly, subcompressed. Beaks obtuse, moderate, near anterior end, with small impressed lunule in front below them. Hinge straight, about two-thirds width of shell. Anterior portion of shell convex; posterior portion slightly flattened, compressed. Anterior end broadly rounded; posterior end narrower, more pointed, angulated between ridges. Ventral margin regular, subparallel to dorsal. Two faint low diverging ridges run obliquely backwards over posterior slope of valves to posterior end of ventral margin, but are almost indistinguishable near the beak. Hinge area behind beaks narrow, lanceolate. Ligament external, in groove. Hinge-line edentulous? Pallial line simple, no sinus. Posterior adductor scar well marked, about half-way between beak and posterior extremity. Anterior scar faint. Surface of valves marked by faint concentric growth-lines.

Measurements.	mm.
Length	15
Width	24
Thickness	8

Remarks.—The characters of the hinge and absence of the typical teeth would alone be sufficient to remove this species from the genus *Ctenodonta*; and it appears to belong to the genus *Orthonota*.

Orthonota solenoides (Sow.)[1] bears a distant resemblance, but is relatively more transverse, and the posterior ridges are wanting.

ACTINOZOA.

Heliolites cæspitosa, Salter. (Pl. XVI, Figs. 6, 7.)

1873. *Heliolites cæspitosa*, Salter: Cat. Camb. Sil. Foss. Woodw. Mus., p. 104 (*a* 343).
1888. *Heliolites cæspitosa*, Etheridge: Cat. Brit. Foss., pt. i (Palæoz.), p. 20.
1891. *Heliolites cæspitosa*, Woods: Cat. Type Foss. Woodw. Mus., p. 20.

The three original specimens (*a* 343) come from the Wenlock Limestone of Dudley and belong to the Fletcher Collection. The species is not uncommon there, and we have numerous specimens from the same locality. Salter (op. cit. supra) describes it as "something like *H. Grayi*, but cells approximate; shallow pits, not raised." One of the original specimens is a fine and almost perfect corallum.

Diagnosis.—Corallum ramose, but not diffuse, consisting of several subparallel, irregularly rounded or lobate, stout branches, increasing in thickness from the base, and bearing calices over their whole surface. Larger corallites small, very numerous, approximate,

[1] Murchison Siluria, 5th ed., 1872, pl. xxiii, fig. 9.

1

2

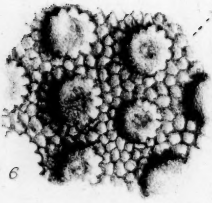
6

3

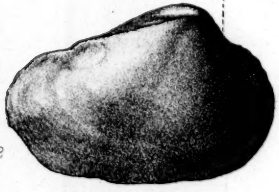
×2

5

× 1½


7

× 6


4

× 1½

G.M.Woodward del.et lith. West, Newman imp.

Silurian Mollusca & Coral.

usually less than half their diameter apart, in some cases almost contiguous; margin slightly exsert, but calyx sunk below general surface; walls crenulated and infolded to form twelve short septa, extending inwards from one-quarter to one-third the diameter of the calyx. Smaller corallites (cœnenchyma) not numerous, usually only 2–4 irregular rows between the larger corallites; polygonal in shape, with well-developed walls and regular horizontal and mostly equidistant tabulæ, so as to divide the tubes into a series of square cells.

Measurements.	mm.
Diameter of calyx of larger corallite	1–1·5
Length of corallum	80·0
Diameter of corallum at base	20·0
Diameter of corallum at crown	55·0

Remarks.—This species differs from *H. Grayi*[1] in the rounded or lobate form of the branches, and in the number, approximation, and depression of the calices of the larger corallites. From *H. inordinata* it differs by possessing stouter, more lobate branches, more numerous, closer and larger corallites with depressed calices, and less abundant cœnenchyma. A Norwegian species named *Plasmopora ramosa*, Kiär, has been recently described[2] which bears a considerable resemblance to *H. cæspitosa*, and is stated to be closely allied to *H. Grayi*. It differs in the more exsert margins of the corallites, the absence of septal prominences, and in the wider spaces between the corallites. The internal structure, moreover, which is the most important feature, appears to be distinct.

EXPLANATION OF PLATE XVI.

Figs. 1, 2.—*Goniophora grandis*, Salter. Wenlock Limestone: Dudley. Fletcher Coll. Nat. size.

Fig. 3.—*Modiolopsis mimus*, Salter. Upper Ludlow: Lesmahagow. Enlarged $\frac{3}{1}$ nat. size.

Figs. 4, 5.—*Orthonota Hughesi*, Salter. Lower Llandovery Beds (Upper Bala, Salter): Sefin Llettyrhyddod, Llandovery. Fig. 4, side view; Fig. 5, hinge aspect of shell. Enlarged $1\frac{1}{2}$ nat. size.

Fig. 6.—*Heliolites cæspitosa*, Salter. Wenlock Limestone: Dudley. Fletcher Coll.

Fig. 7.—Portion of corallum, enlarged $\frac{3}{1}$ nat. size.

III.—On the Making of a Quartz Schist.

By John Parkinson, F.G.S.

In a paper published[3] some eight years ago, Prof. T. G. Bonney discriminated between the dominant characteristics of quartz schists from the Alps and quartzites modified by pressure. The distinctions as therein pointed out may be thus summed up.

In a quartz schist we find an absence of indications of original fragments, the shape of the quartzes proving that they were formed *in sitû*; together with no evidence "that the mica in its present form is derivative"; from which it almost necessarily follows that

[1] Edwards & Haime: Brit. Foss. Corals (Palæont. Soc.), p. 252, pl. lviii, fig. 1.

[2] Kiär: Die Korallenfaun, Ét. 5, Norweg. Silur., p. 32, t. v, figs. 5–6: Palæontographica, Bd. xlvii, 1899–1900.

[3] Geol. Mag., Dec. III, Vol. X (1893), p. 204; also Geol. Mag., Dec. IV, Vol. III (1896), p. 400 ("On a Pebbly Quartz-Schist from the Val d'Anniviers").

the origin of the rock is "a matter of conjecture" rather than of proof (p. 209, etc.). In a quartzite the contrary holds. The clastic origin of the quartzes is obvious, and the form and mode of occurrence of the mica shows it to be detrital; hence the characters separating normal types of the two groups of rock are clear enough in a thin section, if not in a hand-specimen.

The rocks which form part of the subject of the present communication were collected from the neighbourhood of the Illecellewaet Glacier in the Selkirk Mountains; and microscopically present a structure intermediate between that of a quartz schist and that of a quartzite.[1]

Since the gap which separates these two rock types is one which is seldom bridged, I venture to think it may be profitable—(1) to consider the changes which have taken place in the North American rocks which will be referred to as quartz felspar grits; (2) to compare the latter with some of the older quartzites of India, which present indications of change of a rather different kind; and (3) to see what help the whole affords in elucidating the difficult problem—the making of a quartz schist.

1. The Quartz Felspar Grits.[2]

These form a group of rocks exhibiting slight variations in degree of coarseness and manner of fracture, commonly speckled with obvious grains of felspar and quartz; although the sheen surfaces, formed through the development of secondary mica, record the pressure which the rock has undergone. In a few localities, e.g. the moraine of the Illecellewaet Glacier, elongated ellipsoidal spots are conspicuous, exhibiting a parallel arrangement, and which average in size ·5 in. × ·15 in. × ·05 in. As seen in a thin section they consist of very irregular grains of calcite, outlined by an iron staining, which straggle through the other constituents instead of forming a clearly defined patch. These may possibly be the remains of worm burrows distorted by pressure.

[1] See also a paper by Professor T. G. Bonney in Quart. Journ. Geol. Soc., 1888, vol. xliv, p. 32, on the Huronian Series in the neighbourhood of Sudbury (Canada); a sedimentary series showing changes which approximate them in character to a true schist. I am greatly indebted to Professor Bonney for opportunity to study his slides of these rocks, and for suggestions given from time to time on this and kindred subjects.

[2] The age of these rocks is a matter of interest. The identification is a rather difficult one, and I am indebted to Dr. Whiteaves and Dr. H. M. Ami, of the Canadian Geological Survey, for kindly replying to my question on the subject. To the E.N.E. of, and about eighteen miles as the crow flies from, the Illecellewaet Glacier, a Lower Cambrian fauna has been found, and to the westward lies the Shuswap Series, probably of Archæan age. Above the latter and to the east are "dark argillites passing into micaceous schists" (Summary Rep. 1890–1, N.S., v, pt. 1, p. 20A). These are the Nisconlith Series (Lower Cambrian), probably equivalent to the Bow River Series. See also papers by the late Dr. G. M. Dawson, Bull. Geol. Soc. Amer., vol. ii, pp. 165–176, "Note on the Geological Structure of the Selkirk Range," and Ann. Rep. Geol. Surv. Canada, 1896, vol. vii, pt. B, pp. 28B–34B, in which the quartzites in the vicinity of the Glacier House, Mountain Sir Donald, etc., are placed in the Cambrian system under the name of the Selkirk Series and overlying the Nisconlith Series.

The remnants of original mineral grains are conspicuous in most of the thin sections. The presence of felspar, both orthoclase and plagioclase, together with quartz, indicate either a quartz felspar grit or a quartz felsite. The great preponderance of quartz and the very clastic appearance of the felspars clearly point to the former rock; in fact, in many instances the names of quartzite and quartz felspar grit are still applicable, in spite of considerable mineralogical and structural modifications.

The condition of the felpars is the most interesting feature. Mineralogical change is made manifest by a cloudiness in the interior of the crystal, due to the presence of innumerable minute scales, flakes, or prisms of a mineral which, no doubt, is a mica. Towards the periphery of the crystal the latter pass into a fringe of well-formed flakes intermingled with grains of quartz, the character of the former being identical with that of the typical quartz schists of the Alps. Occasionally a system of lines of greater change, and probably of incipient fracture, traverses the crystal, and is marked by closely aggregated scales and flakes of mica. At the same time crush shadows form, followed in due course by fracture and loss of uniform polarization; while, frequently, more or less clear spots appear representing developing individuals of quartz or secondary felspar; the outcome being complete disintegration and reconstitution.

Some of the larger quartz crystals have exceedingly irregular edges and exhibit undulose extinction; and in the smaller grains a similarity of size and of polarization tints is often characteristic, together with some elongation in the direction of foliation. In a more advanced stage of reconstruction a more marked parallelism exists between the flakes of mica. While these are for the most part spread through the rock in thin and irregular bands, they are also embedded in the quartz grains, or wedged between their edges, and have even trespassed the boundary between two individuals. In some sections they reach ·009 inch in length. The presence of a mica flake in the centre of a quartz grain, which is large in proportion to it, and situated near a system of fine cracks by which this grain is traversed, indicates the passage of the mica substance in solution by capillary action. The cracks contain specks of a mineral (about ·0025 inch across) which may be referred to mica. Now and then such flakes appear where no obvious cracks exist, and occasionally a long thin wedge of mica flakes penetrates the quartz grain. The condition of the mica is different to that of the mineral produced by the mere crushing of a quartz felspar grit; the distinction consisting in this, that in the rocks under consideration the mica forms definite flakes usually bounded by definite lines, whereas simple crushing produces a matted intergrowth of film-like scales which ordinary powers and ordinary thin sections fail to resolve into its components. The smaller quartz grains—commonly ·004 to ·009 inch across—possess angular or subangular outlines, are not conspicuously elongated at right angles to the direction of pressure, and do not exhibit undulose extinction to any

noticeable degree. Hence the strain produced by the pressure the rock has undergone has been removed from the greater part of the rock, while the larger and obviously clastic grains still bear its marks.

The rock does not appear to have undergone serious deformation since it assumed its present condition, and as regards the state of the quartzes at the time of maximum pressure we are met with evidence of two and diverse kinds. Firstly, we find clear indications of incipient cracks, conical in section but with slightly concave outlines, which are usually characteristic of mineral matter stressed beyond its elastic limit, and pointing to the crushing of a practically rigid solid; whereas, secondly, the irregular shape of the quartzes under the microscope are suggestive of a plastic deformation, e.g. some have been apparently bulged out by an intruding finger of the rock, or the like.

On the whole I conclude that the constituents of the rock have been partially plastic under the stress to which they have been subjected, but at the same time sufficiently rigid to have cracked in the process.

With the assumption of these characters disappears the smashed-looking aggregate so universally indicative of crush. The mica clearly has been formed *in sitû*, and the production of the quartz mosaic by crushing and subsequent reconstitution could not be safely predicated simply from an examination of this part of the section. The grains of felspar scattered throughout the slide are plentifully sprinkled by minute crystals of mica, or slightly discoloured by brownish dust, but otherwise are quite translucent, and show well-defined polysynthetic twinning (12° to 15°). We find, then, almost a new rock; the traces of original quartz grains are few in number; the mica has been formed *in sitû* and does not suggest a clastic origin; and the greater proportion of the felspars have undergone reconstruction.

2. The Quartzites of Delhi and Jaipur.

Of great antiquity [1] and greatly altered, the old quartzites of India near Delhi and Jaipur yet show no resemblance to the rocks above described. A specimen taken from the celebrated 'Ridge' at Delhi consists of quartz and thickly scattered, exceedingly small crystals of various minerals. The most common of these are shapeless flakes and scales of a quite colourless mica, averaging roughly ·003 inch across, distributed without any orientation through the rock, commonly at the junction of two or more quartzes. Indications of the basal cleavage are usually wanting, and there is no evidence that the mineral is detrital. In addition, we find flakes of a gummy brown to greenish brown tourmaline (and possibly some of brown mica), many of which are exceedingly minute, often not more than ·0006 inch in diameter, and which usually exhibit rounded outlines, especially in the smaller grains. Other constituents occur, but are

[1] They are a member of the transition system, i.e. they occupy an intermediate position between the gneisses and the older Palæozoic rocks.

not easy to identify, including, however, calcite, zircon, specular iron, and pyrites. Between crossed nicols we realize the shapelessness of the quartz grains, which interlock the one with the other as irregular polygons without any trace of an orientation. They average ·019 inch across. A slice taken from the very similar quartzite of the Kutub, 11 miles south of Delhi, possesses less white mica and associated minerals, and the quartzes are dustier and exhibit outlines still more fantastically irregular.

As in the preceding slide, although we find indications of undulose extinction, this is the only sign of pressure, so that any crushing of the grains which may have occurred has been almost entirely obliterated. Now and then, indeed, we seem able to recognize that the longer axes of the grains have a common direction; but the haphazard cross-sections show outlines sometimes almost of a horse-shoe shape with waved edges, at others elongated with many irregular projections. The quartzes contain many rounded spots of felspar; these are unstriped, rather opaque, and often roughly rectangular in section, exhibiting then an obscure orientation. So far as it goes this last-named feature indicates rearrangement *in sitû*. An average size is ·008 × ·003 inch. A few brown zircon prisms and some crystals of magnetite are found. A specimen from the top of the ascent to Ambér from Jaipur also contains felspar fragments, in many of which polysynthetic twinning (extinction 15°) is still discernible, but which also occur as rounded grains included in a quartz crystal, and often are better defined than happens in the preceding slide.

The quartz contains rows of inclusions and gas cavities which have a common direction and occasionally extend without interruption from one grain to another. Strain shadows are inconspicuous, but the parallel lines of inclusions indicate that the rock has been subjected to a certain degree of pressure. The felspars, however, appear not to have suffered in the least, and we find no attempt to break up into mica and quartz. The slide contains also grains of bluish-green tourmaline up to ·016 inch in length, magnetite, zircon, and a few elongated flakes of a green mica.

These rocks form a group usually possessing a saccharoidal lustre and close-grained texture, and are distinguished microscopically by the absence of evidence of pressure and by the irregular outline of the quartz grains. These outlines must naturally have been acquired *in sitû*, the structure of the whole proving great alteration.

Conclusions.

The recent researches of Messrs. Adams and Nicolson on the 'Flow of Marble'[1] are of considerable interest in the present enquiry.

In these experiments stumpy columns[2] of Carrara marble were enclosed in tightly encasing jackets made of a material possessing

[1] "An Experimental Investigation into the Flow of Marble": Phil. Trans., 1901, vol. 195 A, p. 363.

[2] An inch or rather less in diameter and about 1·5 inches in length.

a high elastic limit and considerable ductility. The marble, which alone was subjected to pressure, was shorter than its casing, and was squeezed by means of plugs passing into the ends of the jacket; pressure being continued until the bulging of the latter was carried almost to the point of rupture. The rock was squeezed under three sets of conditions—in the first at normal temperature and dryness; in the second heated to 300°–400° C.; and in the third permeated by water vapour at a pressure of 460 lbs. per square inch, the heating being the same as in the second series. The following results appear significant.

1. The crushing load of the deformed marble of the first series was considerably less than that of the natural rock; due, it is thought, to the production of 'cataclastic structure' along certain lines; in the second series the crushing loads were nearly equal, and in the third series that of the deformed rock was at least equal[1] to that of the natural. Messrs. Adams and Nicolson remark that in these experiments the presence of water was not observed to exert any influence.

The absence of cataclastic structure in series two and three and the high crushing load of the deformed rock indicate that the constituent grains of the marble have suffered no loss of elasticity, but that the conditions were such as to allow different molecular groupings as the pressure increased.

2. The experimenters conclude that under the second and third sets of conditions the marble "has not been crushed in the ordinary sense of the term," but that deformation has been principally produced by slipping on the gliding planes of the component crystals.

3. The shape and character of the grains thus produced closely resemble those of a schist.

In accord with these results is the conclusion reached by Messrs. Ewing and Rosenhain[2] in describing the deformation of certain metals, that "each crystalline grain changes its shape through slips occurring within itself, and its position through slips occurring in other grains"; and they point out elsewhere[3] that, if the adaptability of the grains to accommodate themselves to the increasing pressure by this means is insufficient they will be "driven into and through one another"; a result readily conceivable in the case of such a material as lead, but which, in the widely different substances usually considered by the geologist, seems to require something more than mere pressure.

The 'Upper Slate Member' of the Penokee Iron-bearing rocks (Huronian) described by Irving and Van Hise[4] is a case where the work of pressure appears to have been reduced to a minimum. Here we have the production of a new rock from a greywacké, a quartz or mica schist in which no trace of a clastic origin remains.

[1] Sufficient experiments were not made to determine this point.

[2] Proc. Roy. Soc., 1899, vol. lxv, p. 90.

[3] In an experiment on "the effect of very severe strain on lead": Phil. Trans., 1901, vol. 195 A, p. 285.

[4] U.S. Geol. Surv. Monographs, xix, 1892, p. 296.

Van Hise concluded, moreover, that the heat produced by any superincumbent mass of strata, i.e. mere burying, is inadequate to produce the results observed.

That the conditions imposed on the marble by Messrs. Adams and Nicolson may be imitated in Nature with sufficient nearness to produce closely similar results, may be taken as established; but it does not follow necessarily that such conditions imposed upon a quartzite would result in the production of a quartz schist. The pronounced cleavage, the habit of twinning, and the softness of calcite allow of a comparatively easy adaptation to altered circumstances; hence marble no doubt readily shows the effects of pressure and heat.

Not only so, but we have frequently to deal in Nature with a rock which instead of being a homogeneous aggregate of grains consists of two or more minerals, the physical properties of which are very different. Apply to such great pressure, accompany it by heat, and limit freedom of movement, then we should expect greatest plasticity to be found in that constituent whose point of fusibility was most nearly approached; and rupture, in varying degree, in the remainder. Since a quartzite or a quartz felspar grit is an obdurate rock, we must infer pressure and temperature to be increased beyond the limits assigned them in Messrs. Adams and Nicolson's experiments, if similar results are to be produced. A cataclastic structure is not a characteristic of a true schist; hence we must rely on heat, and heat plus water, to play the greater part in the reconstruction; while to pressure may be imputed the flattening of the individual grains and the foliation.

In the Delhi quartzites we appear to possess an example of the work of the two former agents, in the Canadian grits of all three combined.

IV.—A Vindication of Bacon, Huxley, Darwin, and Lyell.

By Arthur R. Hunt, M.A., F.L.S., F.G.S.

THE recent attempt I have made to show that the absolutely unanimous adverse criticism of Dean Buckland has arisen from a misapprehension of the facts of the case, has led me to enquire further whether the same cause may not account for far more important results in other similar cases, e.g., the attacks of Huxley on Bacon; of Lord Kelvin on Huxley; and of Lord Kelvin and Professor Sollas on Lyell: attacks which cannot fail to lead the unlearned to believe, not that the critics are right, but that all are wrong together, and unworthy of attention.

Having been geologically educated from first to last on the principles of Bacon and Lyell, I have been genuinely mystified by the intense aversion of Huxley to Bacon, as evidenced by a remark made in 1878: "I have been oppressed by the humbug of the 'Baconian induction' all my life, and at last the worm has turned." Now, if ever there was a genuine Baconian it was Huxley himself, as the following extracts taken in order of succession from Bacon's "Novum Organum" will suffice to prove:—

"Experience is by far the best demonstration, provided it adhere to the experiment actually made."

"We must first, by every kind of experiment, elicit the discovery of Causes and true Axioms, and seek for Experiments which may afford light rather than profit. Axioms, when rightly investigated and established, prepare us not for a limited but abundant Practice, and bring in their train whole troops of Effects."

"Fruits and Effects are the sureties and vouchers, as it were, for the Truth of Philosophy."

"Let no one expect any great progress in the Sciences, unless Natural Philosophy be applied to particular sciences, and particular sciences again referred back to Natural Philosophy."

"The Reverence for Antiquity and the authority of men . . . . and general unanimity, have retarded men from advancing in Science, and almost enchanted them."

"Truth is rightly named the daughter of Time, not of Authority."

"Natural Philosophy has, in every age, met with a troublesome and difficult opponent: I mean Superstition."

"In these mixtures of Divinity and Philosophy the *received* doctrines of the latter are alone included, and any novelty, even though it be an improvement, scarcely escapes banishment and extermination."

"In short, you may find all access to any species of Philosophy, however pure, intercepted by the ignorance of Divines."

"In the habits and regulations of . . . Universities . . . everything is found to be opposed to the progress of the Sciences . . . anything out of the common track can scarcely enter the thoughts and contemplation of the mind."

"In forming our axioms from Induction, we must examine and try, whether the axiom we derive be only fitted and calculated for the particular instances from which it is deduced, or whether it be more extensive and general. If it be the latter we must observe, whether it confirm its own extent and generality by giving surety, as it were, in pointing out new particulars, so that we may neither stop at actual discoveries, nor with a careless grasp catch at shadows and abstract forms instead of substances of a determinate nature; and as soon as we act thus, well authorized hopes may with reason be said to beam upon us."

If the above sentiments were discovered anonymous and couched in more forcible language, it is not impossible that they might be attributed to the owner of the well-known initials T. H. H.

There is good evidence that Huxley failed to grasp the teaching of Bacon. For instance, in criticizing Bacon, Huxley observed: "The desire for 'fruits' has not been the great motive of the discoverer" (Life and Letters, vol. i, p. 486). As we have seen above, fruits and effects are not the objects of research, but the sureties and vouchers for the Truth of Philosophy, just as a correct solution of a problem is a surety of the truth of the calculation.

Then again: "Those who refuse to go beyond fact, rarely get as far as fact" (loc. cit.). Now Bacon is ever insistent on teaching

that you must go beyond your facts to your 'axiom'; when, if your axiom is sound, it will carry you on to new facts, or, in Bacon's words, will point out 'new particulars.'

The last quotation, which explains how an axiom is formed from Induction, exactly describes the method by which Darwin formed his great axiom of 'Natural Selection.' What Bacon termed an 'axiom,' Darwin termed a 'theory'; but Huxley strangely always called it an 'hypothesis,' which forty years ago meant no more than an 'assumption.' Now Darwin's theory and Bacon's axioms were far removed indeed from assumptions. It is important to note that the axioma of Science "is that which is assumed as the basis of demonstration," while the axioma of Mathematics is "a self-evident proposition." As Bacon talks of axioms "when established," he infers that they are not always so fortunate; whereas a mathematical axiom is assumed to be established beyond contradiction.

That Huxley should have preached against the Baconian Induction and Experimental Philosophy, has no doubt given rise to a flood of unsound guesswork, and necessitated the Baconian warning: "Nor can we suffer the understanding to jump and fly from particulars to remote and most general Axioms . . . we must not . . . add wings, but rather lead and ballast to the Understanding, to prevent its jumping or flying, which has not yet been done; but whenever this takes place we may entertain greater hopes for the Sciences."

But Huxley, in spite of his heretical preaching, was a staunch Baconian, and never himself lacked the lead and ballast; indeed, in 1894 he wrote: "The *a priori* road to scientific, political, and all other doctrine is H.R.H. Satan's invention—it is the intellectual broad and easy path which leadeth to Jehannum. The King's road is the strait path of painful observation and experiment, and few they be that enter thereon" (L. & L., vol. ii, p. 383).

It would thus seem that Huxley's attack upon Bacon was owing to a misconception of the Baconian Philosophy. Until I read Huxley's aforesaid attacks I had taken the Experimental Philosophy second-hand, from the practice of such men as Darwin, De la Beche, and Lyell; but on essaying to read for myself, I found all the popular editions out of print, and had to purchase a fancy volume valued for its typography. Under these circumstances one is tempted to wonder how many of the critics, of the Huxley school, have even read the "Novum Organum." Some years ago I wrote a somewhat combative paper on "Professorial Research," believing the subject to be original! whereas the "Novum Organum" *opens* with the remark—"They who have presumed to dogmatize on Nature . . . . in the professorial style, have inflicted the greatest injury on Philosophy and Learning." Max Müller, writing in the *Nineteenth Century* in June, 1894, observed that in Germany, "For a philosopher who does not belong to the professorial caste to gain a hearing is extremely difficult . . . . the outsider does not exist." Bacon obviously was an amateur in thought as well as in deed, a rank outsider. Strangely enough, Huxley was equally

strong; e.g., "As for government by professors only, the fact of their being specialists is against them . . . . unfortunately, there is among them, as in other professions, a fair sprinkling of one-idea'd fanatics, ignorant of the commonest conventions of official relation, and content with nothing if they cannot get everything their own way"! (Life and Letters, vol. ii, p. 312).

Kelvin v. *Huxley.*

In an address to the Victoria Institute in 1897 Lord Kelvin, referring to an observation of Huxley in 1869 regarding the age of the Earth—viz., "Most of us are, I expect, Gallios who care for none of these things, being of opinion that, true or fictitious, they have made no practical difference to the earth during the period of which a record is preserved in stratified deposits,"—suggested that Huxley's indifference was due to his ignorance that there was valid foundation for estimates worth considering as to absolute magnitudes. But Lord Kelvin entirely ignored Huxley's reply in 1876, when he pointed out that should Lord Kelvin tell him his geological authority was quite wrong, his answer would be, "That is not my affair; settle that with the geologist, and when you have come to an agreement among yourselves I will accept your conclusion" (Coll. Essays, vol. iv, p. 135). No man could say more than this.

Kelvin v. *Darwin.*

Lord Kelvin quotes from Jukes' "Students' Manual" as follows: "Mr. Darwin, in his admirably reasoned book on the origin of species . . . estimates the time required for the denudation of the rocks of the Weald of Kent . . . *at three hundred millions of years*" (Trans. Vict. Inst., vol. xxxi, p. 12).

This Lord Kelvin thinks very foolish, but so did Darwin; for in 1866 he writes to Professor Charles Pritchard, F.R.S.: "That is a very foolish episode of mine about the Wealden, and was struck out in the later editions" (Life of Professor Pritchard, p. 94).

Not only was the passage quoted by Lord Kelvin struck out, viz., "In all probability a far longer period than 300,000,000 years has elapsed since the latter part of the Secondary period," but the whole two pages which referred to the Wealden were rejected.

Kelvin v. *Lyell.*

"Led by Hutton & Playfair, Lyell taught the doctrine of eternity and uniformity in geology" (loc. cit., p. 16).

Lord Kelvin here again entirely ignores Huxley's refutation in 1876, twenty-one years before the old charge was furbished up again, viz., "It is clear that the consistent working out of the uniformitarian idea *might* lead to the conception of the eternity of the world. Not that I mean to say that either Hutton or Lyell held this conception—assuredly not; they would have been the first to repudiate it" (Coll. Ess., vol. iv, p. 52).

Then listen to Lyell himself. "It cannot be denied that our failure to detect signs of them [the vertebrata] in older strata, in proportion to the rank of their organization, favours the doctrine

of development, or at least of the successive appearance on the earth of beings more and more highly organized . . . . " (Elements, 1865, p. 586). And again, " It may or may not be true . . . . that the whole planet was once in a state of liquefaction by heat " (loc. cit., p. 90).

There is neither eternity nor uniformity in these passages, petrological or palæontological. It seemed to me such a serious thing that such baseless charges against our most illustrious geologists should not only be brought before a scientific institution, but be sold at a cheap rate to the public, that I sent in my resignation as an associate of the Victoria Institute.

For my own instruction I sought to discuss Lord Kelvin's address with a distinguished geologist and teacher, but he refused either to read the address, to permit me to read extracts, or to discuss it in any way, on the ground that Lord Kelvin was not a geologist, and that it would be an absolute waste of time to discuss his opinions on geology. I then applied to another distinguished man whom Lord Kelvin had criticized, but he had not even taken the trouble to ascertain what had been said of him. In fact, I was not only driven from the geological judgment-seat, but bantered for taking the matter so seriously.

Under these circumstances I attended the Meeting of the British Association at Bradford. To my unbounded consternation I heard my friend Professor Sollas, President of Section C, in the opening passages of his address repeat Lord Kelvin's charge against Lyell, with the addition of a serious reflection on the latter's honesty. After excusing Hutton's ignorance the distinguished President went on to say: "With Lyell, however, the case was different: in pressing his uniformitarian creed on geology he omitted to take into account the great advances made by its sister sciences, although he had knowledge of them, and thus sinned against the light. In the last edition of the famous 'Principles' we read: 'It is a favourite dogma of some physicists that not only the earth but the sun itself is continually losing a portion of its heat, and that as there is no known source by which it can be restored we can foresee the time when all life will cease to exist on this planet, and on the other hand we can look back to a period when the heat was so intense as to be incompatible with the existence of any organic beings such as are known to us in the living or fossil world . . . . a geologist in search of some renovating power by which the amount of heat may be made to continue unimpaired for millions of years, past and future, in the solid parts of the earth . . . . has been compared by an eminent physicist to one who dreams he can discover a clock with a source of perpetual motion and invent a self-winding apparatus. But why should we despair of detecting proofs of such regenerating and self-sustaining power in the works of a Divine Artificer?' Here we catch the true spirit of uniformity; it admittedly regards the universe as a self-winding clock, and barely conceals a conviction that the clock was warranted to keep true Greenwich time " (Sollas, Rep. Brit. Assoc. Bradford, p. 712).

On this passage the charge of sinning against the light is founded. Now it is a matter of no sort of importance whether Lyell was right or wrong, but it is a matter of grave importance to his reputation whether he either weakly deceived himself or wickedly deceived other people.

Sinning against the light was certainly not the character of Sir Charles Lyell, as is well shown by his prompt renunciation of his old principles in favour of Darwinism and the antiquity of Man. Pengelly writes in September, 1862: "In his new book he [Lyell] says he is 'going the whole hog' both in the antiquity of man, and Darwinism, and if any man deserved excommunication he thinks he certainly will" (Life of W. Pengelly, p. 135).

It is always lawful for a man to defend his position until it is proved untenable; and the question is, whether the doctrine of the stability of the solar system and the maintenance of the sun's heat was so thoroughly disproved as to make its continued acceptance scarcely honest.

It is worth noticing that in a very popular book, "The Orbs of Heaven," 1859, by O. M. Mitchell, Director of the Cincinnati Observatory, the following passage occurs: "I see the mighty orbits of the planets slowly rocking to and fro, their figures expanding and contracting, their axes revolving in their vast periods; but stability is there. Every change shall wear away, and after sweeping through the grand cycle of cycles, the whole system shall return to its primitive condition of perfection and beauty" (loc. cit., p. 125).

Lyell died in 1875. In 1882, seven years later, C. W. Siemens, President of the British Association, an electrical expert, propounded the very selfsame electrical self-winding clock theory; for which Lyell has been so severely blamed, not for propounding, but for merely suggesting.

Siemens writes as follows: "In March last I ventured to bring before the Royal Society a speculation regarding the conservation of solar energy," which he proceeds to briefly state, and subsequently proceeds: "If chemical action and reaction can further be admitted, we may be able to trace certain conditions of thermal dependence and maintenance, in which we may recognize principles of high perfection, applicable also to comparatively humble purposes of human life" (Siemens, Rep. B.A. Southampton, p. 33). These theories may or may not be sound, but they were boldly announced from the Chair of the British Association as well as defended before the Royal Society.

And yet again, twenty years later, in 1902, from the most fashionable scientific rostrum in London, that of the Royal Institution, we hear Mr. Wells saying: "Some day this earth of ours, tideless and slow moving, will be dead and frozen, and all that has lived upon it will be frozen out and done with. There surely man must end. That of all such nightmares is the most insistently convincing. And yet one doesn't believe it. At least I do not. And I do not believe in these things because I have come to believe

in certain other things—in the coherency and purpose in the world and in the greatness of human destiny. Worlds may freeze and suns may perish, but there stirs something within us now that can never die again" ("The Discovery of the Future," T. G. Wells, *Nature*, 1902, p. 531).

If the Royal Institution listens respectfully in the twentieth century to the unsupported belief that man will survive his frozen globe, and the British Association listened respectfully to Siemens' speculations on such thermal maintenance as would avoid that cold contingency, must Lyell be deemed as sinning against the light if he did not despair of the detention of proofs of a self-sustaining power, the position taken by Lyell being far less confident than that of either Siemens or Wells?

It may be worth while to notice an admirable summary by Phillips of the state of geological theory in 1839: "That the doctrine of progressive cooling of our globe is to be now received as an established theory, those who desire the real progress of geology will prevent themselves from affirming; and perhaps few who have attended to the inferences contained in these volumes will hesitate to believe that it will one day become so. . . . . The figure of the earth, its density, the actual temperature of its surface and interior parts, etc., are all capable of explanation by this one consideration" (Phillips' Treatise on Geology, vol. ii, p. 277).

The earliest teaching I received in geology, some time in the fifties, was, that the figure of the earth, the oblate spheroid, proved its original liquidity; and that catastrophe was "the forlorn hope of an almost extinct body of philosophers." Lyell's Uniformitarian theory was the protest against a series of creations and catastrophes. Like a good cobbler he stuck to his last and left cosmogony to the astronomers and physicists, who alone were competent to deal with it, or at least were supposed so to be. But I must say I have my doubts. Bacon's test for a sound theory is that it shall be able to confirm its own extent and generality by giving surety in pointing out new particulars. If the rule be sound it will prove itself in the example. Now Lord Kelvin's 'Age of the Earth' breaks down completely in his own hands when put to this test. He has worked us an example of his own choosing, wherein, so to speak, every figure is wrong.

Based on his hypothesis of the mode of consolidation of the Earth is Lord Kelvin's conjecture of the 'Probable Origin of Granite,' e.g.: In the primeval lava there arises "a snow shower of solidified lava or of crystallized flakes, or prisms, or granules of felspar, mica, hornblende, quartz, and other ingredients. . . . This process goes on until, by the heaping of granules and crystals on the bottom, our lava ocean becomes silted up to the surface . . . at the stage now reached [we have] a red hot or white hot surface of solid granules or crystals with interstices filled with the mother liquor still liquid, but ready to freeze at the slightest cooling. . . . It was probably this interstitial mother liquor that was destined to form the basaltic rock of future geological time" ("The Age of the Earth," Trans. Vict. Inst., vol. xxxi, pp. 24, 25).

It will be observed that Lord Kelvin's granite is not necessarily even a plutonic rock. Its original crystals of mica, felspar, quartz, hornblende, and other minerals are brought together by gravitation, and have invariably a basaltic matrix. *Ex hypothesi*, it can never be otherwise. Now the questions arise whether a granite of this description has ever been seen by any petrologist, and does it accord with the microscopic character of any known granite? But, granting for the sake of argument the possibility of the formation of such a primeval granite; to what entirely different origin are we to attribute the numerous non-primeval granites in which gravitation, drifting, and surface consolidation are out of the question, and yet granites of different ages are practically indistinguishable the one from the other? Lord Kelvin's theory not only assumes that all the granitic minerals crystallized out of the basaltic magma simultaneously, but that all sank at the same rate, and were therefore all heavier, and equally so, than the residual liquor; whereas, assuming that the granitic minerals, mica, felspar, and quartz, could crystallize out of a magma largely composed of augite and iron, they would tend to rise rather than sink, and these granitic minerals would have consolidated at the surface instead of the bottom. But, as it is an established fact that the granitic quartzes and felspars consolidated under great pressure, in the presence of water in some form, it is clear that granite was formed in some other way than by a shower of crystalline granules.

Geologists have hesitated to propound a 'Theory of Granite,' knowing the difficulties; therefore, Lord Kelvin's theory holds the field. Now, Lord Kelvin's 'Probable Origin of Granite,' and of Basalt, occupies exactly two octavo pages. Within that small space the word 'must' occurs eight times, the word 'probable' or 'probably' six times, 'may have' twice, 'pretty sure' once, 'almost certainly' once. Any one of these words or phrases would suffice to cast suspicion on a new theory; but it is not a case of one, but of eighteen. Such a doubtful 'axiom' must be founded on not a few fallacies, and here are two of them. Lord Kelvin assumes that because consolidated basalt is heavier than molten basalt, and will therefore sink in its own liquor, every mineral crystallized out of the basaltic liquor will also sink. But as is well known, though some minerals are heavier than the liquor, others are lighter. Secondly, Lord Kelvin overlooks the fact that lava, frozen on the surface of a lava-lake, would often entrap bubbles of expanded gases, and be therefore very much lighter than the main mass of the lava, whether solid or liquid. Some of our cosmogonists would do well to refresh their geology by a reference to Lyell on the subject of pumice.

As Lord Kelvin's 'Theory of the Age of the Earth' seems based on the false hypothesis that consolidated basalt and crystallized minerals must necessarily sink in liquid lava, the failure of the hypothesis, or underprop, involves the collapse of the theory. Pengelly's assimilation of the Baconian teaching was much as follows. Every theory is based on many facts, or supposed facts. If a single one of these supposed facts is disproved the whole theory

fails. Lord Kelvin assumes that the earth consolidated throughout from within outward, and that the surface was the last to harden. Mineralogy, Petrology, and Geology say—No.

By way of recapitulation I must submit that the charges levelled respectively against Bacon, Huxley, Darwin, and Lyell have all failed, and for the following reasons.

Huxley objected to Bacon only theoretically. In practice he followed him. The Baconian system of working from experience to axiom, from axiom to experience, and back again to axiom, until the latter is established, is the only safe road, and a safe road it is.

Lord Kelvin's attack on Darwin, for an error acknowledged and corrected some thirty-six years previously, is a species of attack which no reputation can withstand.

Lord Kelvin's attack on Lyell is sufficiently repelled by Huxley.

Professor Sollas's charge against Lyell, of what almost amounts to *mala fides*, is disposed of by showing that men of repute have held doctrines even more unorthodox without their good faith being questioned.

I may perhaps be allowed to say that I have myself been compelled to attempt the solution of problems of the most varied character, for none of which I was fitted by previous training; and that under these adverse circumstances I have never known the Baconian method to fail.

Before undertaking the investigation of the age of the Devonshire schists I had never so much as seen a section of schist. After my paper on Ripplemark was in print I heard for the first time of the existence of Airy's standard work on Tides and Waves; and as for petrology, I have never had a couple of hours instruction in my life, to my own infinite loss and regret. Pengelly used to say: "Be careful in scientific enquiries that you get a sufficient number of trustworthy facts, that you interpret them with the aid of a rigorous logic, that on suitable occasions you have courage enough to avow your convictions; and don't be impatient if your friends don't receive all your conclusions, or even if they call you hard names." This is safe advice, and it may be added that if by any possibility two entirely distinct lines of enquiry can be induced to point to the same fact, that fact is probably trustworthy, whereas if three agree it most certainly is so. For instance, if after a gale a dredge indicates a wave-rippled bottom, and if the fauna present are specially adapted to resist waves, and if mathematical theory asserts wave-action at the particular depth—experience, zoology, and mathematics agree, and there is no doubt the bottom is rippled. If, in addition, the ripples can be experimentally reproduced by proportionate waves at a proportionate depth, experience, experiment, zoology, and theory all agree. It is rarely enough that a fact can be assured by four harmonious cross-bearings, but two cross-bearings, if accurate, are sufficient, whereas three are conclusive. For instance, when geology, zoology, and chemistry agree independently as to the age of the Cambrian rocks, or as to the salinity of the sea at that epoch, that much desired information will have been obtained.

In the meantime it is an object still more to be desired, that scientific men should discontinue the practice, first introduced by Huxley, of prosecuting research by the mutual application of 'beak and claws,' as otherwise the only possible effect on the world in general will be the one represented by the following astounding observation made to me by a highly educated man of the world: "Scientists are a disagreeable lot, *not worth controverting*"!

So far as my own observation goes, science has never been at so low an ebb in the estimation of the general public as at present, unless they fancy they see 'money in it.' During the last five years science has been struck four staggering blows. Two have been already mentioned, but perhaps nothing can equal in danger the attack made on British science and the old Universities by the President of Section B at Glasgow, this being coupled with a panegyric on the German methods of teaching and research, together with scathing contempt for the two English Colleges which have ever aimed at the highest attainable standard of education, Trinity and Balliol. Really, one would think Cambridge had done fairly well with such men as Kelvin, Stokes, Rayleigh, Airy, Sedgwick, Bonney, Teall, and Balfour, and scores of others who have made their marks, though not always such deep ones. At the present moment the claims of science are being pressed on the unscientific public in a remarkable book entitled "Anticipations," in which the average intelligent Englishman is severely handled.

It is scarcely fair, but I cannot resist quoting Bacon's judgment on Anticipations and Interpretations. Here it is: "We have accustomed ourselves to call the one method the '*Anticipation of the Mind*,' and the other the '*Interpretation of Nature*.'" He refers to the former method as follows: "And as for those who prefer and more readily receive the former . . . . because they are unable from weakness of mind to comprehend and embrace the other (which must necessarily be the case with by far the greater number), let us wish that they may prosper as they desire in their undertaking, and attain what they pursue." Readers will kindly note that this appalling sarcasm is Lord Verulam's, not mine.

REVIEWS.

I.—A Text-book of Geology. By Albert Perry Brigham, A.M., Professor of Geology in Colgate University. 8vo; pp. 477, with 294 illustrations. (London: Hirschfeld Brothers, Ltd., 1902. Price 6*s*. nett.)

THIS is a clearly written introduction to geology, full of information and yet devoid of detail, and as well fitted as any book with which we are acquainted to stir up interest in the science and to provide the general reader with a knowledge of the principles and leading results of geological investigation. The work is divided into three parts dealing with Dynamical, Structural, and Historical Geology. After a brief introduction of but two pages, the author

starts on the geological work of winds. In the course of his remarks he points out the useful lessons that may be learned from the disc of earth lifted by a falling tree, from the ridge and hollow thereby formed, from the erosion that may result from the exposure of the soil to wind and water, or from the obstruction that may arise to drainage. The tearing up to a depth of four or five inches and scattering of soil in one district, or the formation elsewhere of rugged cliffs of calcareous sandstone by wind-drifted beach deposits made up of fragments of shell and coral, afterwards solidified through the cementation of the particles, are again vividly pourtrayed. Illustrations are also given of the loess in China with its dug-out human habitations. Referring to the subject of weathering, and to the minor agent of electricity, mention is made of an irregular train of fulgurites leading off from a tree which was struck by lightning in Florida. The erosive and transporting powers of rivers are dealt with, and the alluvial deposits are traced from their sources in cliff and talus to estuarine deposits and bars. Glaciers, lakes, oceans, and volcanoes receive lucid treatment, and all the subjects are illustrated by diagrams and excellent pictorial views. The illustrations, indeed, are drawn from all parts of the world.

Structural Geology is dealt with briefly, the definitions being in one or two instances too concise, as when we are told that "Hydraulic limestone is so called because when ground it will 'set' under water." From a consideration of the minute structure we are led on to contemplate the "gross structure" of rocks—stratification, folds, and faults, veins, bedded ore, placer deposits, and other phenomena. In the chapter on Physiographic Structures various types of mountains are explained, as well as valleys, lake basins, the development of a land surface in youth, maturity, and old age, and the evolution of drainage.

In the section on Historical Geology the principal formations are briefly described, with especial reference to America, while the fossils are treated, not with respect to the latest nomenclature, but in a broad, general way: thus, a "Carboniferous Crinoid" or "Jurassic Crustacean" is illustrated, while other organisms are generically or specifically designated. Some of the figured fossils are well-known British forms, but the references to physical conditions and economic deposits are essentially American. General questions of correlation are treated in a philosophic spirit, and if we miss any special reference to palæontological zones, and to some other topics, we can without hesitation heartily commend this book to the favourable attention of our readers. H. B. W.

II.—A Bibliography of Stonehenge and Avebury. By W. Jerome Harrison, F.G.S. (Wilts Archæol. and Nat. Hist. Mag., vol. xxxii, December, 1901.)

THIS is a quarto work of 169 pages containing the titles of 947 books, papers, etc., by 732 authors, including 143 who have hidden their identity and appear as "Anon." The authors date from Herodotus, B.C. 450, to Sir Norman Lockyer and Dr. F. C.

Penrose, 1901, whose recent observations indicate that the date of the erection of Stonehenge may be about 1680 B.C. Short abstracts or summaries of the contents of the different books and papers are given, and the work contains some photographic plates of Avebury and Stonehenge. Every important reference to the subject, scientific and literary, appears to have been noted, as may be judged when we mention that George Borrow's "Lavengro," Hardy's "Tess of the D'Urbervilles," and other works of similar character are included in the Bibliography. Geologists will be interested in the works dealing with the character and origin of the materials used in the "Great Stone Monuments of Wiltshire." The subject is a many-sided one, and all interested will feel gratitude to Mr. Harrison for his laborious and, it is needless to add, most carefully executed work. H. B. W.

III.—The Sea-Coast. By W. H. Wheeler, M.Inst.C.E. 8vo; pp. 361, with 39 illustrations. (London: Longmans, Green, & Co., 1902. Price 10*s.* 6*d.* nett.)

In this work the author gives the results of his long experience on the coasts of England, Belgium, and Holland, with especial reference to the protection of the land from the ravages of the sea. As an engineer charged with the design of important works for coast-protection, he has given particular attention to the laws which govern the action of waves breaking on the shore, and the effect produced by them on the beach, and on sea-walls and groynes; he has likewise devoted much study to the conditions under which material is drifted along the coast.

In many of the subjects thus dealt with the geologist is more or less interested, especially in the waste on many parts of the coast where "The fight between sea and land is continuous and unceasing." In some localities the cost of preservation of the cliffs may be greater than the value of the land; but as man is sometimes an agent of destruction it is satisfactory to know that when the removal of beach material is shown to be injurious to the coast, the Board of Trade issues an order prohibiting further material being taken away.

From a geological point of view some of the statements of the author require revision, as they lack clearness. Thus, referring to the flint shingle on the South Coast, he remarks that "Many are quite angular, and are still coated with the white colour of the chalk from which they are derived," having, we presume, the white siliceous crust characteristic of chalk flints.

Again, there is a vagueness about the sentence "It seems improbable that the extensive banks of shingle at Aldborough and Weybourne, each several miles in length, can have accumulated from any waste of adjacent cliffs now going on. The present source of supply is derived almost entirely from the waste of the cliffs, and the falling on to the beach of gravel originally deposited on their surface at the breaking up of the glacial period." There is much pebbly gravel in the cliffs at Weybourne, and still more in those of Southwold and Dunwich, from which supplies have been

derived for the shingle beach at Aldborough and Orford. Again, in the description and explanation of the Chesil Bank we find that the geological problems are by no means clearly brought before the reader. Thus we are told that "The greater proportion of the flints of which the bank is composed denote an inland rather than a sea-cliff origin, and seem to point to the time of the breaking up of the last great Ice Age, when the valleys and rivers [*sic*] were scooped out by the ice, and the floods due to its melting, and when vast quantities of rock débris and gravel were carried to the coast."

Those interested in the geological explanations of the beach will do well to turn to the original works on the subject, and it would have been better had the author referred to Mr. Strahan's Geology of the Isle of Purbeck and Weymouth (Mem. Geol. Survey, 1898), rather than to his remarks in a discussion on a paper by Dr. Vaughan Cornish. Mr. A. R. Hunt has carried on many important researches on wave action and marine deposits, and a fuller reference to his labours might have been made. He is mentioned in the text, but not in the index. The paper by Mr. Bristow and Mr. Whitaker on the Chesil Bank is noted under the names of H. W. Burton and W. Whitaker.

Fuller and more precise references to original sources of information on our coasts and coast erosion would have added much to the value of this work. As a record of the author's personal observations on the methods of protecting our sea-border, and as a record of the varying characters of our beach deposits, the work will doubtless prove of considerable value and interest both to engineers and geological students.

IV. — Geology of the Argentino-Chilian Cordilleras. By Dr. Carl Burckhardt. Part II. Profils géologiques transversaux de la Cordillère Argentino-Chilienne. Folio. (La Plata, 1900.)

THE present work (issued by the Museum of La Plata, under the direction of Dr. F. P. Moreno) embodies the stratigraphic and tectonic results of an expedition made in 1897 by Dr. Burckhardt and Dr. Leo Wehrli, the latter contributing a sketch of the physical geography of the region, which exhibits many interesting glacial and volcanic features.[1]

The rocks dealt with range from the Middle Lias to the Danian, and without any distinct evidence of intermediate erosion. The Jurassic rocks have long been known to form an important part of the region, and Dr. Burckhardt finds in them, especially in the Opalinian, Tithonic, and Berriasian, as well as in Lower Neocomian divisions, clear evidence of an Alpine facies in the fauna. Many forms in the Lower Oolites belong to the littoral zone, and the facts in general do not favour Neumayr's views on climatic distribution.

No evidence of Bathonian has been obtained, but there are strata which may represent this age, although they have not at present

[1] A preliminary report by the authors was published in 1897.

yielded fossils; and the same to a certain extent is the case with Oxfordian (as limited by the author) and Rauracian, though we should in this country from the presence of *Cardioceras Lamberti* recognize portions of our Oxfordian. The strata which occur below the Kimeridgian are largely formed of gypsum, red marls, grits, and conglomerates, and they indicate at any rate somewhat altered conditions.

The identification, or approximate identification, of many South American fossils with European species is comforting, for it enables comparisons to be made much better than if distinct 'specific' names had been given; and as the sequence of the forms is the same in the two areas we cannot doubt that approximate correlation is justified. There appears to be a blending of Kimeridgian and Tithonic forms in the Lower Tithonic.

The following table shows the grouping and the leading fossils noted by Dr. Burckhardt:—

Neocomian and Berriasian	... ...	*Exogyra Couloni*, Defr. *Lytoceras* cf. *subfimbriatum*, d'Orb. *Hoplites* aff. *occitanicus*, Pict.
Tithonic	Upper ...	*Odontoceras permulticostatum*, Steuer.
	Lower ...	*Oppelia perlævis*, Steuer.
Kimeridgian	... ...	*Perisphinctes colubrinus*, Rein. *Perisphinctes polyplocus*, Rein.
Rauracian Oxfordian	... ...	(Not yet distinguished by fossils.)
Callovian ...	... ...	*Cardioceras* aff. *Lamberti*, Sow. *Peltoceras athleta*, Phil. *Perisphinctes funatus*, Opp.
Bathonian	... ...	(Not yet distinguished by fossils.)
Bajocian ...	... ...	(Upper zones not yet distinguished.) *Sonninia* aff. *Sowerbyi*, Mill. *Amusium* (*Pecten*) *personatum*, Goldf. *Harpoceras opalinum*, Rein.
Lias ...	Upper ... Middle ...	(Upper beds united with Lower Bajocian.) *Amaltheus spinatus*, Brug.

Between the Neocomian and the Senonian there are strata which have furnished no determinable fossils, but as the formations do not exhibit discordance the author thinks the sequence may be complete. In the Senonian *Gryphæa vesicularis*, Lam., and *Plicatula* aff. *multicostata*, Forbes, are noted; and in the Danian, *Turritella Soaresana*, Hartt, and *Cardita Morganiana*, Rathb.—the *Turritella* being near to *T. ventricosa*, Forbes, and *T. multistriata*, Reuss.

A remarkable fact about the region is that the strata are comparatively little disturbed and plicated; and it thus differs greatly from Alpine regions in the absence of dislocated portions of formations and of any central massif of ancient rocks. In some respects the structure is more like that of the Swiss Jura, from which, however, it is distinguished by the presence of diorites and of extensive neo-volcanic rocks. In the Jurassic series, ranging from the Lias to the Tithonic, there are conglomerates, of more or less porphyritic character, volcanic breccias and tuffs, which appear to replace the strata at various horizons. These rocks will be described in detail in the petrographic portion of the work.

The present volume is illustrated by thirty-two plates of fossils, geological sections, and views, including many effective pictures of glacial and volcanic phenomena as well as of the stratigraphical features in the sedimentary rocks. H. B. W.

V.—The Student's Handbook of Stratigraphical Geology. By A. J. Jukes-Browne, B.A., F.G.S., etc. Large crown 8vo; pp. xii, 589, with 1,000 Illustrations. (London: E. Stanford, 1902. Price 12*s.* nett.)

THAT there must be a fairly constant demand for textbooks on geology is evident from the almost perennial succession of them, mostly by more or less well-known geologists. There is an average sameness about them all: allowing for personal idiosyncrasies, their stratigraphy is 'correct,' while their palæontology, especially as regards nomenclature, is hopelessly out of date, and their illustrations only too frequently as venerable as their subject. Judged on these lines, this, the latest comer of the long series, is a successful production.

The present work, the author tells us in his preface, is a rewritten edition of his "Student's Handbook of Historical Geology," which was issued in 1886 as one of the celebrated "Bohn's Series." The change of title in the present edition more accurately conveys the scope of the work, which does not deal, as the old designation might have led one to infer, with the history of the science itself.

The general plan of the original book has been retained, but the chapters dealing with the Palæozoic rocks have been expanded, and the sections on Palæogeography abridged, since this latter subject has been more fully dealt with in the author's "Building of the British Islands."

Some previous knowledge of geology is indispensable to the student of this volume, and for this he is referred to "any elementary manual of geology" or "the author's *Handbook of Physical Geology*."

The nomenclature and classification of strata adopted by Mr. Jukes-Browne do not materially differ from those employed in other modern textbooks, he having wisely abandoned sundry variations which he proposed in 1885 (Geol. Mag., 1885, p. 297) and incorporated in the former edition of this book. He does, however, introduce the term 'Selbornian,' coined by him in 1900 (Geol. Surv. Memoir, "The Cretaceous Rocks of Britain," vol. i), to include the Upper Greensand and Gault; and for this term there is some justification.

A glance at a modern table of strata brings to mind, with amusement, Ramsay's "Lament for the Good Old Days of William Smith," when

> "Our ancient English was the law
> In geologic volumes."

We have advanced far indeed in the importation of foreign terms since then.

After the introductory portion comes the main body of the work, in which each system is made the subject of a chapter.

As usual in treatises of this class, but scant courtesy is paid to the post-Pliocene deposits, especially the non-Glacial ones. They generally come last, and, like the final letters in an encyclopædia, or the unfortunate Invertebrata in many a so-called Natural History Book, there is no room left for adequately dealing with them. Dare we suggest that the greater difficulty in correctly unravelling their history as compared with their more showy precursors in geologic time has also something to do with the case? Anyhow, the refusal to grant them a 'system' name to themselves is, we think, for many reasons to be regretted.

The order adopted in treating of each system is to give, first, the Nomenclature and Classification, then come the Life of the Period, the Stratigraphy, the Physical Geography, and finally a list of References. Where they lend themselves to it, these sections are appropriately, though we feel bound to say not always satisfactorily, illustrated; while a full index concludes a well got up volume so far as regards paper and print. The illustrations are of very unequal merit. The new maps and sections are most admirable, and so too are some of the figures of fossils. Far too many of these latter, however, are from old and worn-out blocks, like the figure of *Homalonotus* on p. 141, which first did duty so many a long year ago that we scarce like to compute its age; while others, like the cut at the top of the same page, looking like some post-Bank-holiday fragment of feminine adornment, scarcely depict the objects they ostensibly represent. We are thankful to record, however, that the pig-tailed Mammoth with his wooden shoes is conspicuous by his absence.

On the whole Mr. Jukes-Browne is to be congratulated on the attempt he has made to grapple with a difficult subject, and we imply no disrespect to him if we say that it is only too manifest a work of this description can in the future only be successfully brought out if produced under the auspices of a combination of specialists.

A geologist who has worked all his life at stratigraphy has had no adequate opportunity of making himself properly acquainted, for the purposes even of a work of this sort, with the advance of knowledge in the sister sciences of zoology and palæozoology, botany and palæobotany.

Mr. Jukes-Browne will do well, therefore, when preparing future editions, to take counsel with experts in these several sciences. Guided by them, he will see the advisability of making many modifications, such as the breaking up of the list of mammals at present banded together as "New-comers in the older Thames Gravels," and certainly the removal therefrom of the Roman introduction, *Cervus dama*. He will learn that more than one worked flake has been found in the lower brickearth of the Thames Valley below London; and so on, and so on. He will further be assisted in bringing his nomenclature of the fossil forms of life somewhere within the range of modernity.

Even as it is our author might here, of himself, have improved upon what he has done. A little reflection would have convinced

him that the emended form '*Vivipara*' was more appropriate than '*Viviparus*,' seeing that so far as known at present male animals do not produce young, and that anyhow *Viviparus vivipara* could not be right.

There are unfortunately numerous nomenclatural inconsistencies to puzzle the student, that should have been eliminated. The worst of these is the treatment meted out to the quondam genus *Ammonites*. Mr. Jukes-Browne has evidently struggled hard to cope with the correct modern divisions, but he has been worsted in the conflict. He starts bravely enough in the middle of p. 334, where the modern (more or less) generic names are employed; but from the bottom of that page, and for some way further on, these same names are treated as subgeneric and placed in parentheses after '*Am.*' Between p. 340 and p. 350, and beneath all the cuts of the Jurassic forms, however, '*Ammonites*' alone appears; thenceforward for some pages the subgeneric method is again followed. Finally, the strain becomes too great, all three styles are employed indiscriminately, with the result that the same species occurs under different forms of names at short intervals, so that, for example, we read of '*Am.* (*Douvilleiceras*) *mammillatus*' on p. 427, of '*Douvilleiceras mammillatus*' on p. 429, and of '*Am. mammillatus*' on p. 430. Poor student!

B. B. W.

REPORTS AND PROCEEDINGS.

Geological Society of London.

I.—March 26th, 1902.—Professor Charles Lapworth, LL.D., F.R.S., President, in the Chair. The following communications were read:—

1. "On a remarkable Inlier among the Jurassic Rocks of Sutherland, and its Bearing on the Origin of the Breccia-Beds." By the Rev. John Frederick Blake, M.A., F.G.S.

On the coast of Sutherland due south of Port Gower is seen on the scars at low-water a long rocky crest of Old Red Sandstone, with its flaggy beds dipping at a high angle. It is of considerable height, and is surrounded by nearly horizontal Jurassic beds containing large blocks of rocks similar to those of the crest, irregularly placed. The size, outline, and relation to the surrounding rocks show that this cannot be a transported block, but must have been part of, or directly derived from, a neighbouring coast—like the modern sea-stacks of the present coast at Duncansby.

The relations of the Jurassic rocks to the Old Red Sandstone are seen in the Gartymore Burn. The fragments of the latter contained in beds of the former become more numerous as the junction is approached, and ultimately form the whole mass—as would happen in the case of a cliff-talus. It is concluded that the breccia-beds may in part have originated on the spot.

The distribution of the breccia-beds in the Jurassic Series is then considered in detail. Three horizons can be traced in this series,

namely, the zones of *Cardioceras alternans*, *Hoplites eudoxus*, and *Perisphinctes Pallasi*. No breccia-beds are found in the first of these, but there are sporadic ones in the second, within which the inlier occurs, and they become very numerous in the third. The ordinary autochthonous fossils, including plant-remains, occur in the intervening shales, but the breccia-beds themselves contain only large heterochthonous fossils, including Rhynchonellas and corals, and where these occur intermediate beds are found, composed of crushed organisms. North of Helmsdale one breccia-bed has apparently squeezed up the underlying shale into an anticlinal arch, against another boss of Old Red Sandstone.

The phenomena thus described are then compared with those which have been actually seen—or which may be inferred to occur in the case of deposits from an ice-foot—and they are found to correspond in a remarkable degree. And it is therefore concluded that the breccia-beds are the product of an ice-foot of Upper Jurassic age, which invaded the normal deposits of that period.

2. "On a Deep Boring at Lyme Regis." By Alfred John Jukes-Browne, Esq., B.A., F.G.S.

During 1901 a boring was made near Lyme Regis in search of coal, and was carried to the depth of 1300 feet without reaching the base of the Upper Triassic Marls. The following abstract shows the formations passed through :—

		Thickness.			Depth.	
		ft.	in.		ft.	in.
Soil and Gravel		10	8	...	10	8
Blue Lias	probably	62	4	...	73	0
Rhætic Beds.	White Lias ,,	22	1	...	95	1
	Black Shales ,,	38	7	...	133	8
	Grey Marls ,,	39	1	...	172	9
Keuper Marls, 1129 ft. 3 in.	Marls, without gypsum	124	7	...	297	4
	Marls, with veins of gypsum	118	10	...	416	2
	Marls, with beds of gypsum	313	10	...	730	0
	Gypsiferous marls, with three beds of sandstone	134	0	...	864	0
	Hard clays and marls, with gypsum.	297	7	...	1161	7
	Hard silty and micaceous clays, with some gypsum	140	5	...	1302	0

A full journal of the boring is given, and the beds are compared with those exposed along the cliffs from Lyme to Sidmouth. The exposures of the Rhætic beds near Lyme are described, and the 'tea-green marls' are included in the Rhætic group, although no fossils have been obtained from them in Devon. Reference is made to the difficulty of measuring the Keuper Marls. The site of the boring is in the valley of the stream which enters the sea at Lyme, at a spot about one mile north-west of that town and about half a mile east of Uplyme, close to the boundary between Devon and Dorset. The Blue Lias in the borehole belongs to the zones of *Ammonites Bucklandi*, *A. angulatus*, and *A. planorbis*. Between the depths of 480 and 864 feet three beds of grey, calcareous sandstone were traversed, each from 12 to 15 inches thick, and separated by beds of red and grey, gypsiferous marl. Similar beds occur near Taunton

and North Curry, in Somerset. Some of the lowest beds may be called 'siltstones'; they were originally silty muds. The author concludes that the boring did not reach the beds which near Sidmouth form a passage from the Keuper Marls to the Keuper Sandstones, and that the Keuper Marls proved by the boring are at least 1130 feet, and may amount to 1200 feet in thickness.

II.—April 16th, 1902.—Professor Charles Lapworth, LL.D., F.R.S., President, in the Chair. The following communications were read:—

1. "The Carlisle Earthquakes of July 9th and 11th, 1901." By Charles Davison, Sc.D., F.G.S.

The shocks were at least four in number, and there are single records of four other shocks. The isoseismal 5 of the first and principal shock is very nearly a circle 29 miles in diameter, with its centre 7 miles south-south-west of Carlisle, and is excentric with regard to the isoseismal 4. The continuity of the shock over a band extending from Carlisle to Coniston implies a corresponding continuity in the focus. The investigation of the earthquakes has led to the recognition of a deep-seated fault, the average direction of which is N. 5° E. and S. 5° W., and the hade throughout is to the east. In the surface-rocks there is no sign whatever of such a structure. The movements along the fault were somewhat peculiar. In the first shock the focus was of considerable length, and consisted of two principal portions, the centres of which were about 23 miles apart, connected by a region wherein the slipping was continuous throughout, and much less in amount. The northern part of the focus was smaller than the other, but was marked by a much stronger impulse. The third slip was complementary to the first, for it appears to have occupied the whole of the region between the two principal portions of the first focus, and to have been greatest near the centre of that region and to have gradually diminished towards both ends.

2. "The Inverness Earthquake of September 18th, 1901, and its Accessory Shocks." By Charles Davison, Sc.D., F.G.S.

Since the Comrie earthquake of 1839, which was followed by 330 tremors and earth-sounds within little more than two years, no British earthquake has been attended by so many accessory shocks as this one. The unusual intensity of the earthquake, its apparent connection with the great northern boundary fault of the Highlands, and the possibility of tracing oscillations in successive centres of disturbance along the fault surface, combined in rendering a detailed investigation desirable. With a few exceptions, the earthquakes originated beneath the district lying between Inverness and the north-eastern end of Loch Ness. The mean direction of the fault, which follows the line of the Great Glen, is N. 35° E. and S. 35° W., and its hade is to the south-east. The isoseismal 8 contains 67 square miles, and its centre is about $1\frac{1}{2}$ miles east-north-east of Dochgarroch and $\frac{3}{4}$ mile south-east of the fault-line. The correspondence between the position of the great boundary fault

and of the fault inferred from the seismic evidence is so close, that there can be little doubt that the earthquake was due to a slip along this fault.

The nature of the shock, the sound-phenomena, time-relations, and after-shocks are described in detail, and some account is added of the earthquakes of 1890 and of sympathetic earthquakes in the valley of the Findhorn. There were two distinct slips in rapid succession, with continuous slight motion between them, the second being greater in amount and extending over an area which probably overlapped, even if it did not entirely include, that within which the first took place. The great slip reached nearly from Loch Ness to Inverness, and was greatest at a point about half-way between. The three chief after-slips resulted in an extension of this area in both directions along the fault-surface, the extension to the north-east being small, while that to the south-west amounted to 6 miles or more. In addition to this migration of the focus, there was also a continuous decrease in the depth of the focus. The earthquakes provide no evidence with regard to the direction of displacement along the boundary fault. There can be little doubt, however, that Loch Ness is still growing; but it can hardly be determined whether the lake is now contracting in area, or whether it is gradually pushing its way outward to the sea.

3. "The Wood's Point Dyke, Victoria (Australia)." By Frederic Philip Mennell, Esq., F.G.S.

This dyke is intrusive into a belt of Silurian (Upper Silurian) strata which strike in a direction somewhat west of north, and extend beyond Walhalla on the south. Wood's Point is about 75 miles east of Melbourne. It may be taken as typical of the intrusions associated with the Silurian rocks of the Victorian gold-fields. Brown, original hornblende is the dominant constituent, but it is rarely idiomorphic; augite, three varieties of felspar, micropegmatite, and ilmenite are also present in a microcrystalline or cryptocrystalline groundmass. The rock is called a hornblende-porphyrite. In certain varieties cordierite occurs, and is accounted for by derivation from the adjacent shales. The reefs in the Silurian and Ordovician rocks usually occur at or near the contact with intrusive rocks. At Wood's Point the reefs are nearly horizontal, traversing dykes and shales, the junction usually marking the occurrence of rich ore. The author notes the "almost invariable association of gold in this class of deposit with rocks containing original hornblende."

CORRESPONDENCE.

THE CRYSTALLINE LIMESTONES OF CEYLON.

Sir,—As one of the unfortunates who are unable to attend the meetings of the Geological Society, I crave permission to make a comment on the interesting paper on "The Crystalline Limestones of Ceylon" read on the 12th March by Mr. Ananda K. Coomára-Swámy. The author and all his critics profess themselves more or less puzzled

by the phenomena described. No mention is made of a theory which would, I think, remove the chief difficulties mentioned. I suggest that the true explanation is deformation *plus* chemical segregation. The author rejects deformation (1) because the minerals are not deformed. But this is not to be expected in a rock which has undergone advanced metamorphism. The author assumes that the minerals are original, whereas the rock may have been transformed, as in the Malvern Hills, where some of the gneisses are entirely composed of secondary minerals, and show no trace of the deformation which was evident enough in an earlier stage of the metamorphism. The author objects (2) that the minerals whose distribution are the chief cause of the foliation are not "such as to have been produced" by earth-movements. I cannot see why. During the reconstruction of the rock the segregation of the carbonates of lime and magnesia may well have been accompanied by the crystallizing out of the accessory lime and magnesia silicates. The hypothesis I have suggested accounts for three facts which appear to have caused considerable perplexity, viz.: (1) the isolated masses of granulite, (2) the lenticular form of some of the granulite masses (one would like to know if any of the limestones are lenticular), and (3) the subordination of the limestone to the silicate rocks. The theory of the origin of limestones by segregation from plutonic rocks during deformation, as in the case of the crystalline limestones of Bodwrog and Porth Trecastell in Anglesey (Report Brit. Assoc., 1887, p. 706), has not yet, I think, received the consideration it deserves.

C. Callaway.

Cheltenham.

ON THE OCCURRENCE OF *SCALARIA COMMUNIS* IN THE RAISED BEACH ON THE THATCHER ROCK, AND OF *PECTEN MAXIMUS* AND *VENERUPIS*, sp., AT HOPE'S NOSE.

Sir,—In the year 1888 I published in the Transactions of the Devon Assoc. a list of shells found in the Raised Beach on the Thatcher Rock in Torbay.

In due course I sent a reprint to my friend and colleague the late Mr. D. Pidgeon, F.G.S., which Mr. Pidgeon took the practical and ingenious course of returning to me crowded with copious comments and criticisms. As Mr. Pidgeon took the extreme line of denying the genuineness of the Raised Beaches, and I did not feel disposed to seriously entertain this objection, I laid my friend's notes on one side and forgot all about them.

Last Easter, in looking for a copy of my paper for a friend joining the excursion of the Geologists Association to the Gower Caves, I found Mr. Pidgeon's returned copy. In it I discovered a rather important fact, viz., an additional shell to what I believe is already the record list for any one Raised Beach, viz., *Scalaria communis*. Mr. Pidgeon also added his initials to *Aporrhais pes-pelecani*, a species before certified by Mr. J. T. Marshall. *Scalaria communis* makes the grand total of species from the Thatcher beach 44, a total which made Mr. Pidgeon remonstrate, "there are a great

many too many shells, both species and individuals, for a beach." A curious puzzle, but a beach it nevertheless is.

The late Sir Joseph Prestwich embodied my Thatcher collection in his table of the "Mollusca of the Raised Beaches" (Q.J.G.S., vol. xlviii, p. 300).

Anyone interested in the investigation may be pleased to add three species to Sir J. Prestwich's total of 64,[1] viz., *Scalaria communis*, from the Thatcher, on the authority of Mr. D. Pidgeon, and *Pecten maximus* and *Venerupis*, sp., from the Hope's Nose beach, on the authority of the late Mr. Godwin-Austen.[2] Mr. Godwin-Austen described the *Venerupis* as *decussata*, but as that is not a British shell it was probably *V. irus*.

I hope to publish Mr. Pidgeon's criticisms and to discuss them in the transactions of a provincial society. He was one of the acutest of observers, and his detection of the perfect preservation of the sculpture of the Raised Beach shells was one of the most important observations ever recorded on the subject, and one by no means easy to explain.

A. R. Hunt.

Torquay, *May* 8, 1902.

SUB-OCEANIC RIVER VALLEYS.

Sir,—To those who have given some attention to the above subject the following statement may prove interesting.

In the recent work, "The Scenery of England," by Lord Avebury, F.R.S., I had the gratification on turning over its pages to find sympathetic reference to my investigations into the phenomena presented by the submerged river valleys of the British Islands and Western Europe, but coupled with a caution "that perhaps I had carried the argument further than the facts entirely warranted."

In thanking Lord Avebury for his kindness in presenting me with a copy of his book, I referred to this caution, which I regarded as quite natural, and added that it would give me much pleasure to afford his Lordship an opportunity of examining the Admiralty Charts themselves, in order that he might judge for himself whether I was justified in my conclusions as to the position and depth below the surface of the ocean to which these "drowned river valleys" descend.

Accordingly a day was kindly arranged for the examination of the Charts, which Lord Avebury examined with the greatest and most intelligent care, and at the end he expressed his gratification at the result.

His Lordship is now, I understand, preparing a new edition of "The Scenery of England," and a few days since I had the pleasure of receiving from him a letter in which he says: "After looking at your maps I am omitting the sentence in p. 106 in which I express a cautious doubt" in regard to your conclusions. This candid acceptance of my views may, perhaps, have some weight with those geologists who have opposed my views, but who have not taken

[1] Q.J.G.S., vol. xlviii, p. 301, Prestwich writes 64 species, but the list shows only 63.
[2] Trans. Geol. Soc., vol. vi, p. 442.

the only course by which they can be properly tested; that is to say, by tracing for themselves the isobathic contours on the Admiralty Charts by means of the soundings.

Before concluding I wish to call attention to a remarkable corroboration of these views which has recently appeared from another quarter. In his elaborate work on the Glacial Geology of the Christiania Region of Norway, in which the variations of level at successive epochs are elaborated, Professor Brögger[1] says: "the occurrence at great depths of the Norwegian Sea of the high-arctic fossil shallow-water Mollusca of the 'Yoldia fauna' is explained by the hypothesis, that the sea-bottom during the time of the greatest ice-sheet of Europe must have been uplifted at least 2,600 métres higher than it is at the present." It is remarkable that this amount of uprise corresponds very closely with that determined by myself; namely, 1,200 fathoms (7,200 feet) as the elevation of Western Europe during the intensest cold of the Glacial period. The maximum elevation was coincident with the stage of maximum cold of that long period; they were, in fact, cause and effect. We require no other explanation for the cause of the intense cold, and the subsequent changes of climate, than the oscillations of level of land.

EDWARD HULL.

THE UPPERMOST CHALK OF THE BALTIC.

SIR,—In his valuable communication "On some Crustacea . . . from the Upper Cretaceous of Faxe" (GEOL. MAG., Nov. 1901, p. 487) Dr. Henry Woodward has copied from Dr. K. O. Segerberg's paper "De Anomura och Brachyura Dekapoderna inom Skandinaviens Yngre Krita" (Geol. Fören. Stockholm Förhandl., xxii) certain statements concerning the geology of the Baltic Uppermost Chalk (Yngre Kritan). Since these might mislead English readers, I ask leave to correct them.

Since 'Faxekalk' is identical with 'Corallkalk,' it is incorrect to say that "The lower layer of the Faxe Chalk is . . . . largely composed of corals, hence called coral-chalk." Even if the term 'Faxekalk' be applied to the whole of the Uppermost Chalk, the expression is misleading, since the bottom bed of the Uppermost Chalk does not consist of the Corallian Limestone (Faxekalk *sens. str.*), but this latter itself rests on a bed of coccolith-limestone (Saltholmskalk).

The hardness of the coccolith-limestone depends on a secondary cementing through calcite-crystals or flinty matter, a process in no way restricted to any definite horizon; looser varieties of this limestone are found just as much in its lower levels as "in the upper layer."

The coccolith-limestone forms the main facies; here and there on the coccolith-ooze there sprang up coral banks or groves of bryozoa; but the shower of coccoliths proceeded unchecked, so that

[1] "Om de Senglaciale og Postglaciale Nivaforandringer i Kristianiafeltet," pp. 682–3.